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The structure of Zulu morphemes comprises series of open-ended syllables, for example:-

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The status of the nasals' syllabicity is in a state of flux! Thus it is clear that the nasal syllable in

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is a simple CV-syllable. However, in the following examples, this is not so clear:-

umxhentso	V N CV C_3V
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izincwadi	V CV N CV CV

This problem is only of particular consequence in the concords - the prefixes underlined above. As I shall discuss in more detail below, certain rules apply only to syllables; others to all nasals. It seems reasonably clear that m is regarded and used as a full syllable - as in mhlope; in this case therefore preserving the syllable-structure:-

umxhentso V S CV CV where S = CV

However, n has not achieved full-syllable status - thus a rule discussed below specifically prevents vowel-elision under the same conditions that 'mhlope' is realised, for 'inkhulu', etc. That is, 'nkhulu' is not a possible word. This leaves n in an intermediate position:-

It cannot appear as its own syllable;
It is part of the prefix, not the stem, in the example;
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It is deleted as a nasal syllable under certain conditions.

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Section 10 demonstrates that the qualificative remains relationally anomalous, and considers it as a verbal, 'pre-positional', and case-marking particle. Section 11 considers its use in the fourth type of modification, and reconsiders its relation to the third type. Section 12 examines some other general arguments about nominal modification in Bantu languages.

The 'adjective' in Zulu

.1 There is quite a noticeable lack of authoritative writing on the general subject of adjectives, especially in languages generally regarded as 'exotic' by European linguists. In particular, very little seems to have been done to examine closely the precise nature of the 'adjective' in African languages. This study of nominal modifiers (rather than 'adjectives') in Zulu, a Southern African 'Bantu' language, is designed to investigate the relationship between various types of nominal modifier in Zulu, to study problems in their generation, and to relate the conclusions back to some universal principles and to the general study of such modifiers and their semantics.

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a + i → e
a + u → o
a + a → a
a + o → o
u + a → wa
i + a + i → ye

.4 Zulu is described by traditional grammarians as a reflective language, as opposed to an inflective language. That is, the language is characterized by a set of concords, which operate across all the major parts of speech, determined by the noun class of the head noun. Thus the subject noun determines the form of the subjectival concord on the verb, the adjectival and relative concords, the possessive, demonstrative, and enumerative concord. The object noun is likewise related to the verb and to its qualificatives.

4.1 Umu/ntu om/de u/zi/funa izin/cwadi ezim/bili esi/ncane ezim/nyama
man tall want books two small black

Zulu has eight major noun-classes, and a remnant of a ninth from Ur-Bantu. Each class has a singular and a plural class prefix and a distinct set of concords clearly related to these prefixes. See Table 1 - Zulu.

At one stage, the division into classes was semantically very significant; thus Class 3 consisted of trees and plants. Class 1A still consists mainly of major kinship terms and monetary words, Class 7 of abstract nouns. With borrowings from other languages, especially in the Zulu-ising of European words, the distinction has become rather blurred in most of the other classes.

.5 The adjective
Doke (1927) details a very small set of 'adjectives' in Zulu. These he identifies by the form of the concord prefixed to the stem, which concord he calls 'the adjectival concord'. Table 2 - Zulu sets out a complete list of all stems taking this concord. Clearly, some of them are what one would regard as "paradigm" adjectives - 'long', 'short', 'new', 'old', etc. The rule Doke gives for the formation of this concord is as follows:- the qualificative particle -a- is prefixed onto the noun class prefix of the head noun. Thus we get (see Table 1 - Zulu) for example, a + um → om; a + imi → emi. This gives the equivalent of a prenominal adjective or an adjective in a relative clause in English.

Table 1 - Zulu. Concords by noun class.

Noun class	Noun class prefix	Subj. (verb)	Adj.	Rel.	Enum.	Poss.	Obj. (verb)
1 s	umu, um	u	om	o	mu	wa	m
p	aba, (abe)	ba	aba	aba	ba	ba	ba
1A s	u	"	"	"	"	"	"
p	o, (awo)						
2 s	umu, um	u	om	o	mu	wa	wu
p	imi, im	i	emi	e	mi	ya	yi
3s	ili, i	li	eli	eli	li	la	li
p	ama, (ame)	a	ama	a	ma	a	wa
4s	isi, is	si	esi	esi	si	sa	si
p	izi, iz	zi	ezi(N)	ezi	zi	za	zi
5s	im, in	i	em	e	i	ya	yi
p	izim, izin	zi	ezim	ezi	zi	za	zi
5A s	[illegible]	i	em	e	i	ya	yi
p	ama	a	ama	a	ma	a	wa
6 s	ulu, u	lu	olu	olu	lu	lwa	lu
p	izim, izin	zi	ezim	ezi	zi	za	zi
7	ubu	bu	obu	obu	bu	ba	bu
8A	uku, uk	ku	oku	oku	ku	kwa	ku
8B	uku	ku	oku	oku	ku	kwa	ku
(9)	pha	-	-	-	-	kwa	-

All concords are prefixed:-

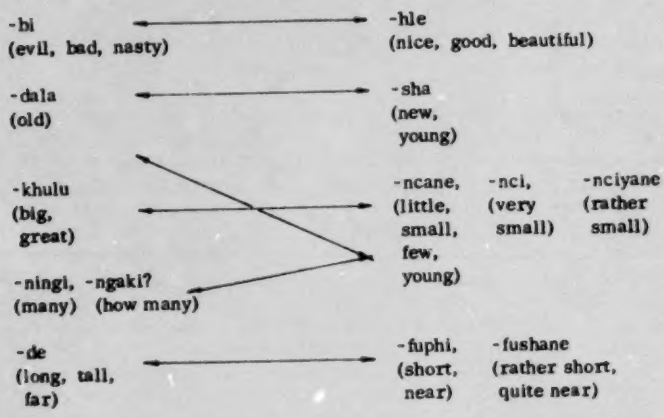
umu-thi	tree	u-thi	stick
imi-thi	trees	izin-thi	sticks
(class 2)		(class 6)	

Thus:-

indoda ende	a tall man, or, a man who is tall.
ubuso obuhle	a beautiful face, or, a face which is beautiful.
abantu abadala	old men, or, men who are old.
imithi emikhulu	large trees, or, trees which are large.

It must be noted that Zulu does not have any articles, and the use thereof in translation is for ease only. The closest translation of "indoda ende" is "man and he tall".

Table 2 - Zulu. All Stems taking the Adjectival concord.



-nye
(other)

-bili
(two)
-th athu
(three)
-ne
(four)
-hlanu
(five)

Note that the same form of opposition as exists between opposite paired quality and quantity adjectives in English is present in Zulu - in Zulu also it is strange to ask "How short are you", etc. The adjective class in Zulu seems to be limited precisely to those items discussing the most important quality and quantity distinctions.

The numbers from six to ten take the Relative concord, another indication of the closeness between adjective and Relative.

Note that, as predicted by Venneman (1972), the adjective in Zulu follows the head noun. (With an exception discussed below). The adjective can also be used predicatively. Doke gives as the rule for the formation of the concord in question the removal of the qualificative -a- and the deletion of the first vowel of all two-syllable concords. Thus (see Table 3 - Zulu):-

5.2 umuntu mkhulu	the man is large
5.3 izwi lihle	the voice is pleasant
5.4 izinkomo zimbi	the cattle are bad
5.5 uthi lukhulu	the stick is big

A question arises; which is, so-to-speak, primary of the relative/ attributive and predicative uses of the adjective? Clearly, the answer given usually for this in English is that the predicative is primary - the adjective in predicative form is embedded under the head NP node prior to the generation of the relative. Doke claims the opposite for Zulu. (p. 221). On purely intuitive grounds, it seems semantically more feasible for the predicative to come first; whatever particular structural relation the particle -a- creates between adjective and noun, it is clear that it is a subordinated role. However, there is a fairly convincing instance which seems to support the contention that the predicative is primary. This will become clearer later, when I show that two morpho-phonemic rules will describe a whole range of concords. If the predicative is regarded as primary, no exception rules are needed; if the attributive, then an exception rule is required for the singular concord of class five. Unlike 5.2-5 above, this concord does not drop its initial vowel:

5.6 indoda inkhulu — the man is large

Given the order of the rules (see Table 4 - Zulu), a separate 'a + vowel' elision rule would be needed (in addition to rule 2). Note that both rule 1 and rule 2 are independently motivated for the formation of the subjectival concord to the verb. Now, even were rule 2 to be amended somewhat along the lines of the present formulation of rule 1, an additional rule would be required for the remaining vowel of the

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ABSTRACT

This issue is devoted to language universals and consists of 4 papers. "Zulu Nominal Modification - An Outline," by A. Johnston, discusses modifiers in Zulu and their relation to nominals. "Shape Classifiers and Natural Categories," by S.G. Pulman, discusses the role of the dimension of "shape" in language classification. "What Might a Universal Theory of Cases Look Like?" by D.A. Kilby discusses the relationship between certain aspects of morphological form and the syntax and semantics of case relationships. "Theoretically Significant Word Order Patterns in Greenberg's (1963) Appendix II," by J.A. Hawkins, argues that this appendix, which contains classifications of languages according to 4 types of word order, provides valuable insights into universal principles of word order. (AM)

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Zulu Nominal Modification - An Outline

Anton Johnston

University of Essex Dept. of Language and Linguistics
Occasional Papers pp. 1 - 33

.0 This outline serves as an abstract for the arguments to follow. Firstly, I discuss points of general relevance to the argument, concerning some details of Zulu phonology and syntax, in particular in relation to the nature of the concord system (sometimes known as 'agreement prefixes', 'harmonisers'). (Sections .1 - .4).

Section 5 discusses the relation between the predicative and appositive use of the Zulu adjective, in particular relating to the form of the concord, and the presence or absence of the qualificative particle. Section 6 discusses Zulu adjective order.

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the simple sentence has been taken to be primary in all languages, so the predicative would seem also to be primary, and this would seem to be supported by the parallel adjectival construction.

8.1 Izimbuzi zimbi	goats are nasty
Izimbuzi ezimbi	nasty goats/which are nasty
Izimbuzi ziqatha	goats are strong
Izimbuzi eziqatha	strong goats/which are strong
Izimbuzi zigijima ...	goats run ...
Izimbuzi ezigijimayo	goats which run

To capture the surface forms of all these six and all their equivalents in all noun classes and tenses, only two rules are required (excluding the mechanisms of derivation of the qualificative particle); I have called these rules morpho-phonemic in that they effect phonological changes but require to refer to syntactic facts and have syntactic effects. The first rule required is to distinguish the adjectival construction from the Relative and verbal. The difference, as mentioned above, is that the adjective retains the full form of the noun class prefix, while the Relative/verbal elides all and only the nasals from the noun class prefix. Thus where the noun class prefix has no nasals, the construction is identical for all three forms.

8.2 isikolo sikhulu	the school is large
isikolo esikhulu	the large school/which is large
isikolo siluhlaza	the school is green (blue)
isikolo esiluhlaza	the green school/which is green
isikolo sithula	the school is shut
isikolo esithulayo	the school which is shut

The precise syntactic significance of the elision of the nasal syllable is very difficult to determine. Where it appears in the noun class prefix, it does not seem to have any particular semantic significance - thus, for example, Class 1 and Class 3 have a nasal syllable in the singular and not the plural, while Class 2 and Class 5 have it in both. Classes 4, 7 and 8 do not have a nasal syllable - 7 and 8 are the deverbal and abstract noun classes, but 4 is a miscellaneous class without significance. Nor is its non-appearance in the verb essential - the only possible source of confusion is with the Class 1s objectival concord (see Table 1 - Zulu). Its syntactic significance is likewise not altogether clear. It is possible to argue that the difference indicated between adjective and Relative is the same as that between noun and verb. There are two possible arguments against this. Firstly, Doke maintains that all Relative stems are nominal in origin, and he provides a fair number of examples to demonstrate this, some of which are not particularly convincing. Thus he claims that '-mnyama' is derived

from 'umnyama', a Class 7 abstract noun meaning "darkness"; whereas the opposite seems probable. Also, the part-of-speech origin of the stem should not materially affect the relation between the derivate and the sentence - this is like claiming 'ability' as a verb. This point of course introduces the quarrel between Interpretive and Generative Semantics, where the latter would try in the course of derivation to relate the underlying part-of-speech to the eventual surface part-of-speech, and the former (see Jackendoff, (1972)) do not allow transformations to perform derivational morphology. As I have pointed out above, in very similar circumstances neither theory makes much progress towards a solution. We incline towards the latter theory as being more 'explanatory' - I shall return to this below.

Secondly, it is clear that in Zulu both Relative and adjective behave in very similar ways syntactically. Thus, for example, consider the following examples:-

8.3 izimbuzi ezindala ezimhlope ezinthathu
goats old white three

8.4 umuntu omkhulu obomvu
umuntu obomvu omkhulu
(a big red man)

8.5 Siyazifuna izincwadi ezimbili ezibomvu ezincane
(Lit: we pres those want books a they two a they red a they small)
We want those two small red books.

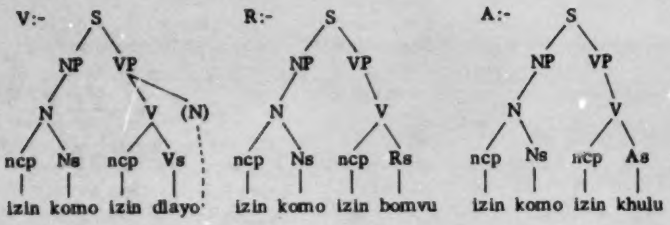
Note: the way in which Zulu expresses the definite as opposed to the indefinite sense of a sentence, or rather, of the object of a sentence, is by use of the objectival concord as demonstrated in 8.5.

Just as a set of adjectives can be used in any order, so a set of Relatives or a set of mixed adjectives and Relatives can be used in any order. This clearly demands that the derivation of the two be treated in very similar ways. It is worth noting that an earlier grammarian treated Relatives as being adjectives, with those few that he notes as having non-nasal stems (such as the Relative -bomvu) as exceptions. (Crout, (1859)). Thus he claimed that a stem like -mnyama, listed by Doke as a Relative, simply "swallowed up" the nasal syllables in the adjectival concord. However, this explanation overlooks the close link between the Relative and the relative/verb, and does not take into account the number of the exceptions.

However, given the clearly very close link between the three concords, it seems a reasonable step to treat them in the same way, and to introduce the difference in concords via a 'late' morphophonemic rule.

Thus the initial structures could be:-

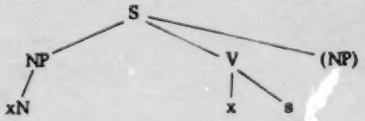
8.6



But the actual form of the embedding will still need extensive consideration.

Nonetheless, the given structures are fairly close to deep structure - we might postulate a deep structure of the form:-

8.7



where 'x' is determined by the class of the noun inserted, 's' is the stem of a 'copulative' in the construction.

The rules required for the differentiation are, firstly, the nasal syllable deletion rule:-

$$8.8 \quad N \rightarrow \emptyset \ / \ \# \ (V_1+)V(S) \ \_\_ \ +]_{vr}$$

which applies to the verb and to the Relative in both 'predicative' and 'relative' positions.

The second is the initial vowel of a bisyllable elision rule required for the predicative use of verb, Relative and adjective:-

$$8.9 \quad V \rightarrow \emptyset \ / \ \# \ \_\_ \ S+]_{avr}$$

These rules are ordered 8.8 - 8.9 which is the order of maximization of application, ie. a feeding order. Application to examples can be seen in Table 4 - Zulu.

.9 An examination of the structure of the use of the qualificative construction and the mechanisms of noun modification demands a closer examination of the particle -a- which appears so prominently as a feature of the 'attributive/relative' in Zulu.

Doke, and Crout, list the particle as a 'possessive', and it is to be found clearly used as the rough equivalent of the English 'of'. Thus the possessive concord (see Tables 1 and 6 - Zulu) is clearly formed in very much the same way as the adjectival or Relative concords (attributive) are formed; closer to the latter, in that it also demands deletion of the nasal syllable. Thus "the man's cow" is expressed:-

9.1 inkomo i+a+in+doda → inkomo yendoda
cow it of man
inkomo i+a+umu+ntu → inkomo yomuntu

9.2 inja i+a+mi na+um+sila u+a+yo → inja yami nomsila wayo
dog it of me & tail it of it
(my dog and its tail)

A similar 'possessive'-type relationship is expressed in Zulu by the use of the particle 'na'. The particle is given three related translations - 'have'; 'with'; 'and'. Thus:-

9.3 Izimbuzi izi+na+u+laka → Izimbuzi zinolaka
goats they have anger

9.4 Izimbuzi a+izi+na +u+laka → Izimbuzi ezinolaka
(goats which are angry/angry goats)

There is clearly a semantic relationship between the two particles, and a (possibly spurious?) morphological relationship too - the difference noticeably being the deletion of the nasal ... Perhaps "a" = 'with', "na" = 'be with'?

One factor to be considered before examining the following sentences which require generation is that the actual possessive must be placed next to the head noun.

9.5 izinkomo izinkhulu
9.6 izinkomo a+izinkhulu a+izimhlope zizidla utshani
9.7 isikolo a+isikhulu a+isi+mhlope sithulwa
9.8 izinkomo a+izinkhulu a+izidlayo zimhlope

9.9 /zinkhulu 'they are big'
9.10 /zi/mhlope 'they are white'
9.11 /zi/yadla 'they eat'
9.12 yizinkomo 'they are cattle'
9.13 /zi/naulaka 'they have anger'
9.14 Umuntu a+u+na+in+ja a+i/fileyo) "The person whose
9.15 umuntu a+in+ja i/a+khe i/file/a+i/file) dog is dead"

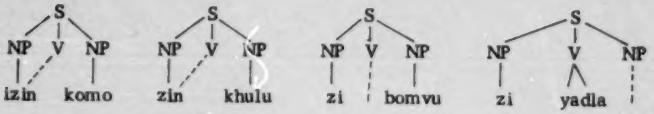
A further piece of interesting data is provided by the prenominal use of Relative and adjectival and possessive stems:-

9.16 obomv'umuntu ; kukhona amahl'amanzi
9.17 omkhul 'umuntu ufikile ; omkhulu ufikile
9.18 ezami zinhle kakhulu

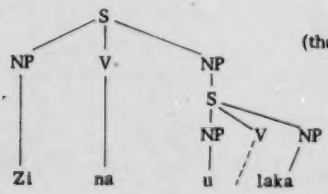
Doke postulates all three as pronouns, therefore translating roughly:- "a red one, a man"; "there is good stuff, water"; "a big one, a man has arrived"; "a big one has arrived"; "mine is very nice".

All of these provide striking evidence for the independence of each completely-formed 'word'; in fact, it seems reasonable to argue that the noun, the Relative, the adjective, the verb, all have sentential status (as in 9.9-13), thus:-

9.19



(they be cows) (they be big) (they red) (they eat)



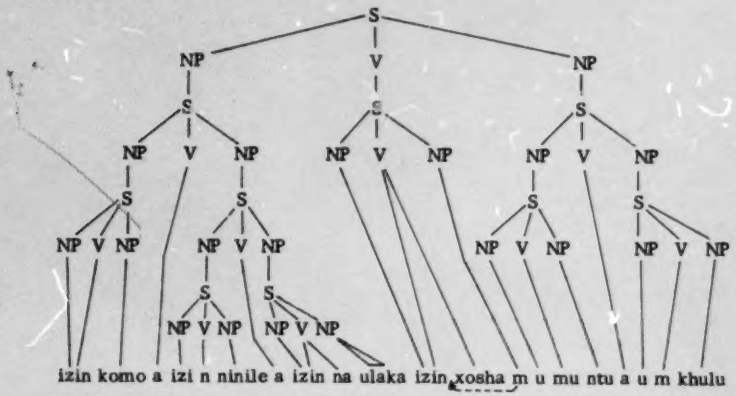
(they have anger)

The last example gives an idea as to the classification of the parts of speech in such a structure - one would need the rewrite conditions

9.20 $S \rightarrow NP\ V(NP)$ $\quad V \rightarrow x$
$NP \rightarrow S$ $\quad NP \rightarrow y$
$V \rightarrow S$

which would yield a most unwieldy deep structure:-

9.21



"the cows which are fat which have anger are chasing him the man big"

There are several points beside the unwieldiness of this structure which are against it. The first is the treatment of the particle 'a' :- even if it can be considered verbal (as Fillmore (1968) on 'and', 'have', 'with'), intuitively, and in Zulu phonology, explicitly, the element is considered as a constituent of the 'relative' which succeeds it; this (9.21) structure relates it to the head noun, as:- 'boots and shoes' rather than 'boots and shoes'. Then, I do not have any precedent at all for a rewrite 'V → S'. Further, other than that Zulu has no apparent present tense of the verb 'to be', that the syllable is deleted in some constructions, and that elements modified by the concord alone can constitute complete sentences, I have no major justification for treating the nasal syllable elided in the Relative and verb as representing a 'verb' element. The treatment seems to offer semantic sense for the adjective: "it is large" and for the verb: "it / eats"; but seems not to make much sense for the Relative: "it / reds", so to speak.* However,

* Note Ross' treatment of English adjectives (1968).

Table 4 - Zulu. Concord derivation.

1. (Nasal elision) $N \rightarrow \phi \ / \# \ (V_1 +) \ V \ (S) \ \_\_ \ +]_{vrp}$

2. (Vowel elision) $V \rightarrow \phi \ / \_\_ \ S \ +]_{avrp}$

3. (Coalescence) # a+i(S)+ — e
a+u(S)+ — u

Verb	indoda	infuna	Adj.	indoda	indala	Adj.	indoda	# a+in+dala
1.		ifuna	Pr. 2.		-	At. 2.		-
2.		-				3.		endala
	imithi	imisha		imithi	imidala		imithi	#a+imi+dala
1.		isha	2.		midala	2.		-
2.		-				3.		emidala
	isikolo	isisha		isikolo	isikhulu		isikolo	# a+isi+khulu
1.		-	2.		sikhulu	2.		-
2.		sisha				3.		esikhulu
	izimbuzi	izimfuna		izimbuzi	izindala		izimbuzi	# a+izin+dala
1.		izifuna	2.		zindala	2.		-
2.		zifuna				3.		ezindala
	ukudla	ukuphelile		ukudla	ukuhle		ukudla	# a+uku+hle
1.		-	2.		kuhle	2.		-
2.		kuphelile				3.		okuhle

Rel.	indoda	inqotho	Rel.	indoda	# a+in+qotho
Pr. 1.		iqotho	At. 1.		# a+i+qotho
2.		-	2.		-
			3.		eqotho
	imithi	imiqatha		imithi	# a+imi+qatha
1.		iqatha	1.		# a+i+qatha
2.		-	2.		-
			3.		eqatha
	isikolo	isiluhlaza		isikolo	# a+isi+luhlaza
1.		-	1.		-
2.		siluhlaza	2.		-
			3.		esiluhlaza
	izimbuzi	izimqatha		izimbuzi	# a+izim+qatha
1.		iziqatha	1.		# a+izi+qatha
2.		ziqatha	2.		-
			3.		eziqatha
	ubuso	ububomvu		ubuso	# a+ubu+bomvu
1.		-	1.		-
2.		bubomvu	2.		-
			3.		obubomvu

N:- nasal syllable, nasal NV, N=n, m; S:- full syllable $C_1 V$, m

6.1 izinkomo ezinhle eziningi ezindala ezinkhulu
cattle a they nice a they many a they old a they big

(or any combination of the four)
cattle nice many old big

English has only one fully acceptable order:-

6.2 many nice big old cattle
?many nice old big cattle

If the adjectives are conjoined, they differentiate between sets of items:-

6.3 izingubo ezindala nezinhle
(among blankets, old ones, and nice ones)
(an exclusive 'and' ?)

Zulu adjectives do not have degrees of comparison; different kinds of construction, diminutives, or separate unrelated lexical items being used.

.7 Given that according to Doke's classification there are so few adjectives, what supplies the deficiency in Zulu? Firstly, Doke identifies three other qualificatives; the enumerative, the possessive, and the "relative" - henceforth Relative.

At first sight, the Relative seems remarkably like the adjective. Doke identifies the Relative by the form of the prefix, which is very similar to the adjectival concord. However, there is one significant difference.

Doke gives two equally unsatisfactory rules for the formation of the Relative concord. The first is that the Relative is formed from the adjectival concord by elision of the nasal syllable from the concord where such a syllable is to be found - in seven of twelve different concords. The second is that the Relative is formed from the qualificative + the first vowel of the noun class prefix + the subjectival concord. Either of these two rules achieves the right results, but both lack any seeming of an explanation as to why. The two definitions however give the following clues:-

1) They link the Relative and the adjective
2) They link the Relative and the subjectival concords
3) They point to the elision of the nasal syllable.

The Relative can be used predicatively - in these cases, those Relative stems which perform qualificative functions (see Table 5 -

Zulu) are prefixed by the subjectival concord; ie. that concord which is prefixed to the main verb in harmony with the head NP. Some examples (see also Table 4 - Zulu):-

7.1	Indoda iqotho	the man is honest
7.2	Indoda eqotho	the man who is honest/the honest man
7.3	Indoda ibona....	the man sees....
7.4	Imithi ibomvu	the tree is red
7.5	Imithi ebomvu	the tree which is red/the red tree
7.6	Imithi isha	the tree burns

Table 5 - Zulu. Some Stems taking the Relative concord.

-banzi (wide)	-ngcono (better)
-qatha (strong)	-duma (tasteless)
-qotho (honest)	
-buhlungu (painful)	-mtoti (sweet)
-bukhali (sharp)	-munyu (acid)
-buthuntu (blunt)	-nzima (heavy)
-lunama (sticky)	-ze (nude)
-lukhuni (hard)	-zibhadu (speckled)
-lusizi (self-sacrificing)	
-makhaza (cold)	
-manzi (wet)	
-mnandi (pleasant)	
-mnene (kind)	
-mpofu (poor)	
-bomvu (red)	-mnyama (black)
-luhlaza (green, blue)	-mpofu (dun)
-lula (light)	-mpunga (grey)
-luthuthuva (cream)	-mpemvu (white-faced)
-mdaka (mauve)	-usundu (brown)
-mdubu (light mauve)	
-nyaluthi (light mauve)	
-mhlope (white)	

On the basis of these examples, we could say that the link between the predicative concord and the subjectival concord is that they are the same; ie. the Relative is being treated as a verb. (In the same way, on

the argument above, we might say that the predicative adjective is being treated as a nominal). The link between the predicative and attributive use of the Relative is that the latter is linked, in ways as yet structurally unexplained, to the former by the addition of the qualificative particle -a-. However, there are some difficulties with this analysis. Consider:-

7.7	Izimbuzi ziqatha	the goats are strong
7.8	Izimbuzi zifuna ...	the goats want ...
7.9	Izimbuza eziqatha	the strong goats/the goats which are strong

The simple addition of -a- to 'ziqatha' should give 'aziqatha'. Clearly there is an extra vowel in the underlying form of the subjectival/ Relative concord - reversing a phonological rule (see .3 above) this is clearly /i/ - which Doke mentions in the second part of his rules for formation of the concord. This means that in the example 7.7-9 the underlying concord form is 'izi', developed in the attributive form by the addition 'a + izi'. The underlying subjectival/Relative concords then become:-

7.10 u aba i isi izi ulu ubu uku ili a

rather than as set out at present:-

7.11 u ba i si zi lu ba ku li a

(See Table 6 - Zulu for a more complete exposition).

.8 There is therefore a clear parallel between the use of the Relative and the use of the adjective in Zulu. The predicative use of the Relative involves addition to the Relative stem of the full subjectival concord; the predicative use of the adjective involves addition to the adjectival stem of the full noun class prefix. The attributive use of both involves the further addition to these respective concords of the qualificative particle -a-.

The argument as to whether the Relative predicative or the Relative attributive is primary does not seem easy to settle on any clear syntactic grounds. By the very appellation 'Relative' the "attributive" would seem to occupy this primary position - especially when it is clear that all verbs in direct relative clauses (where the head NP is the subject of the relative, as in "The boy who saw the end of the world") are introduced by the Relative concord. However, consideration shows the normal relation between subject and verb to be signalled by the subjectival concord, so that the same relation holds between subject and verb, or subject and direct relative, as between subject and Relative predicative, or subject/head noun and Relative attributive. As

Table 6 - Zulu. Underlying concords.

Noun class	Noun class prefix	Subj.	Adj. (pred.)	Adj. (attrib.)	Rel. (pred.)	Rel. (attrib.)	Poss.
1s	umu	u⌐	⌐mu	a+umu=om	u⌐	a+u⌐=o	u⌐+a=wa
p	aba	ba	ba	a+aba=aba	ba	a+ba=aba	ba+a=ba
2s	umu (as 1s)						
p	imi	i⌐	mi	a+imi=emi	i⌐	a+i⌐=e	i⌐+a=ya
3s	ili	li	li	a+ili=eli	li	a+ili=eli	li+a=la
p	ama	a⌐	ma	a+ama=ama	a⌐	a+a⌐=a	a⌐+a=a
4s	isi	si	si	a+isi=esi	si	a+isi=esi	si+a=sa
p	izi	zi	zi	a+izi=ezi	zi	a+izi=ezi	zi+a=za
5s	in	i	in	a+in=en	i	a+i=e	i+a=ya
p	izin	zi	zin	a+izin=ezin	zi	a+izi=ezi	zi+a=za
5As	in (as 5s)						
p	ama (as 3p)						
6s	ulu	lu	lu	a+ulu=olu	lu	a+ulu=olu	lu+a=lwa
p	izin (as 5p)						
7	ubu	bu	bu	a+ubu=obu	bu	a+ubu=obu	bu+a=ba
8A & B	uku	ku	ku	a+uku=oku	ku	a+uku=oku	ku+a=kwa

class five singular concord. On grounds of simplicity, therefore, it seems motivated to postulate the predicative to be primary in Zulu also. This conclusion requires also the demonstration that the two forms are clearly linked. This demonstration is easy to provide - it is, for one thing, implicit in Doke's formulation for the construction of the concords involved.

Consider:-

5.7 uthi lukhulu	the tree is large
5.8 uthi olukhulu	the large tree - the tree which is large
5.9 izinkomo ezimbi	the bad cattle - cattle which are bad
5.10 izinkomo zimbi	the cattle are bad

(Further examples see Table 3 - Zulu)

The difference does seem simply to be the deletion of a coalesced vowel. But viewed syntactically, more is at stake - clearly the noun class prefix and the adjectival concord are closely related in these and other examples. If we postulate that the two are in fact the same, we are left with two parallel syntactic developments, depending on the presence or absence of the qualificative particle:-

A. izinkomo iziNbi ⟶ izinkomo ziNbi
(the exact realisation of the nasal is determined by the adjective-stem initial consonant)
B. izinkomo a + isiNbi ⟶ izinkomo eziNbi
C. inkomo indala ⟶ inkomo indala
D. inkomo a + indala ⟶ inkomo endala

For a demonstration that this works for all concords on adjective stems, see Table 3 - Zulu. The rules required, given this angle of approach, can be simply drafted (see again Table 4 - Zulu), and take the form of morpho-phonemic rules - that is, they have both phonological and syntactic consequences, and have to make reference to the part of speech of the concords involved. I will discuss the structural consequences below.

.6 Unlike English, Zulu experiences no trouble with adjective order. Adjectives can be placed in any order (somewhat as English relative clauses are not ordered after their head NP). Also as with English relative clauses, they are not conjoined in any way. Thus:-

Table 3 - Zulu. Parallel predicate and attributive use of adjectival and relative

N class:	1		2		3		4	
Adj:-								
1. Attr.	omkhulu	abakhulu	omkhulu	emikhulu	elikhulu	amakhulu	esikhulu	eziNkhulu
Deriv:								
qual + Ncl prf	a+um	a+aba	a+um	a+imi	a+ili	a+ama	a+isi	a+izi(N)
2. Pred.	mkhulu	bakhulu	mkhulu	mikhulu	likhulu	makhulu	sikhulu	ziNkhulu
Deriv:								
v.e.aj	ǿm	ǿba	ǿm	ǿmi	ǿli	ǿma	ǿsi	ǿzi(N)
v.e.Ncpj	ý́m	ǿba	ý́m	ý́mi	ý́li	ǿma	ý́si	ý́zi(N)
Sccd.	u	ba	u	i	li	a	si	zi
Rel:-								
1. Pred.	uqotho	baqotho	uqotho	iqotho	liqotho	aqotho	siqotho	zigotho
Deriv:								
Sccd + rel.	u+	ba+	u+	i+	li+	a+	si+	zi+
	u+	ǿba+	u+	i+	ý́li+	a+	ý́si+	ý́zi+
2. Attr.	oqotho	abaqotho	oqotho	eqotho	eliqotho	aqotho	esiqotho	eziqotho
Deriv:								
a+Ncp+ nas.e.	a+uḿ	a+aba	a+uḿ	a+iḿý́	a+ili	a+aḿǿ	a+isi	a+izi(N)
a+Sccd (rev)	a+u	a+ba	a+u	a+i	a+ li	a+a	a+ si	a+ zi
	a+u	a+aba	a+u	a+i	a+ili	a+a	a+isi	a+izi
a+Ncpv +Sccd	a+u+u	a+a+ba	a+u+u	a+i+i	a+i+li	a+a+a	a+i+si	a+i+zi
Ulsccd	u	aba	u	i	ili	a	isi	izi
Ulrel	uqotho	abaqotho	uqotho	iqotho	iliqotho	aqotho	iziqotho	iziqotho
Uladj	umkhulu	abakhulu	umkhulu	imikhulu	ilikhulu	amakhulu	isikhulu	iziNkhulu

N class:	5	6	7	8
Adj:-				
1. Attr.	enkhulu	ezinkulu	olukhulu	ezinkhulu	obukhulu	okukhulu
Deriv:				
qual+ Ncl prf	a+in	a+izin	a+ulu	a+izin	a+ubu	a+uku
2. Pred.	inkhulu	zinkhulu	lukhulu	zinkhulu	bukhulu	kukhulu
Deriv:				
v.e.aj	?ɸ+in	ɸzin	ɸlu	ɸzin	ɸbu	ɸku
v.e.Ncp	in	ɸzin	ɸlu	ɸzin	ɸbu	ɸku
Sccd.	i	zi	lu	zi	bu	ku
Rel:-						
1. Pred.	iqotho	ziqotho	luqotho	ziqotho	buqotho	kuqotho
Deriv:						
Sccd+ rel.	i+	zi+	lu+	zi+	bu+	ku+
	i+	ɸzi+	ɸlu+	ɸzi+	ɸbu+	ɸku+
2. Attr.	eqotho	eziqotho	oluqotho	eziqotho	obuqotho	okuqotho
Deriv:						
a+Ncp+ nas.e.	a+iɸ	a+iziɸ	a+ulu	a+iziɸ	a+ubu	a+uku
a+Sccd (rev)	a+i	a+ zi	a+ lu	a+ zi	a+ bu	a+ ku
	a+i	a+izi	a+ulu	a+izi	a+ubu	a+uku
a+Ncpv +Sccd	a+i+i	a+i+zi	a+u+lu	a+i+zi	a+u+bu	a+u+ku
Ulsccd	i	izi	ulu	izi	ubu	uku
Ulrel	iqotho	iziqotho	uluqotho	iziqotho	ubuqotho	ukuqotho
Uladj	inkhulu	izinkhulu	ulukhulu	izinkhulu	ubukhulu	ukukhulu

concords.

Attr. attributive. Pred. predicative. qual qualificative particle. Ncl prf noun class prefix.
ncpv noun class prefix initial vowel. v.e. vowel elision. nas. e. nasal syllable elision.
Sccd subjectival concord. Ul underlying. Deriv: derivational history (alternatives) of concords.

11.2 ulukhezo lu+a+umuthi (ukhezo lomuthi)
spoon it of wood /"wooden spoon"

11.3 ilizwi li+a+iqiniso (izwi leqiniso)
word it of truth /"true word"

11.4 indlu i+a+ukudlela (indlu yokudlela)
room it of eating /"dining room"

11.5 izindaba zi+a+amanje (izindaba zamanje)
matters they of now /"current matters"

11.6 abantu ba+a+kuqala (abantu bakuqala)
people they of long ago

11.7 abantu ba+a+khona (abantu bakhona)
people they of here /"local people"

A similar set of circumstances has been extensively considered by Tazikala (1974) for another African language, Kijungan. To the few adjectives he assigns a simple as-for-English embedding structure; to possessives a fairly simply related structure. The case for Zulu seems somewhat different in both instances. Consider also:-

11.8 ukhezo lomuthi ludala
"the wooden spoon is old"

11.9 *ukhezo oludala ulomuthi/lomuthi/etc.
"the old spoon is wooden"
(The possessive has to follow the head NP immediately).

11.10*ukhezo olomuthi/ulomuthi/etc.
"the spoon is wooden"

11.11 inja oyomkhulu = inja a + u + i + a + a = umkhulu
dog of him it of to him is bigness
"the dog is the large one's"

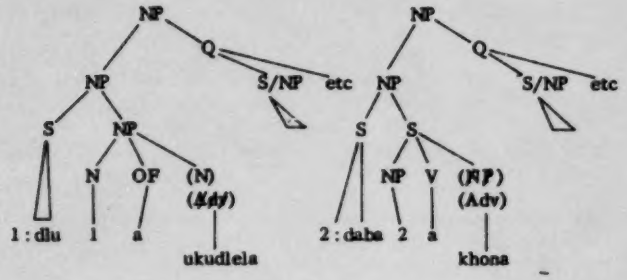
The predicative use of the possessive is rare, and is only found in the possessive of direct relationship; that is, only in the case of "the coat belonging to the man", not as in the case in question, "a spoon of wood". In this case at least, the attributive is primary. Note however the elimination of the nasal syllable in 11.4 and 5, which means (given my assumptions on the reasons for the elision of this syllable) that the possessive "a" is performing a verb-like function in nominal modification, or else that the descriptive possessive is non-sentential. (This invalidates most of Tazikala's structures for use in Zulu). One other feature of the possessive which distinguishes it from the other nominal modification constructs is that it is firmly fixed in order to the head NP.

11.12 inja lenduna endala emnyama
"an old black male-of-a-dog"
*inja endala lenduna emnyama

(As an aside, it is interesting to comment on the close similarity between the Zulu descriptive possessives and the English reference modifiers, de-adverbial and denominal).

Given these differentiating features of the possessive, and using the structure suggested in 10.5, we can postulate the following structures:-

11.13



The joint "NP - Adv" node is further justified by the extreme closeness in Zulu of the adverb and the nominal. (See Doke).

.12 A few questions still require discussion.

(i) Is the Zulu adjective/Relative restrictive or appositive? Hagège (1974) argues that the answer for most African languages is in general that they can only be restrictive. Again the situation seems somewhat different for Zulu. Although with a small stretch of the imagination we could say that all adjectives occur in some form of subordinate relative clause, or as a direct predicate (Hagège's major criteria for restrictiveness), the use of the objectival concord to indicate definite reference means that any adjective following a noun in object position with an objectival concord indicating that noun in the verb is of necessity appositive.

12.1 Ngiyazifuna izincwadi ezibomvu
I they want books red
"I want those very books, which are red"

12.2 Ngiyafuna izincwadi ezibomvu
I want (some) books red

Due to the structure of the Relative or adjective or 'have'-construct, the adverb 'incidentally' may not be interposed as in English as a final test; however, the distinction holds good for informants.

(ii) Hagège also maintains that in many African languages the inviolable order head noun - possessive - relative prevents sentences like "the young hare's small leg". While Zulu also maintains this inviolable order, the possibility of pronominalising the Relative and adjective provides an alternative strategy:

12.3 Omkhul 'umkhonto womuntu omde
big one, spear of man tall

This of course would not work for the 'have'-construct, nor for two possessives:

12.4 *ukhezo lomuthi lomfundisi wesifazane
"the wooden spoon of the woman teacher"

The pronominal use of the Relative/adjective in 12.3 is of course restrictive.

(iii) Hagège says that the major function of the use of the adjective in many languages is the nominalisation of the verb; though other languages have adjectives which are part of the noun system, in which case they are brought into relation with the head noun by an "of"-type particle. Clearly several such parts-of-speech are co-present in Zulu.

(a) The adjective. Hagège identifies as being part of the noun system those adjectives which are preceded by the full nominal concord. This is clearly the case in Zulu.

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(c) The possessive. I have indicated that these do not fit into the general pattern of noun modification in Zulu, and have pointed out that in many ways the construction resembles the English reference modifier. As such, it is hardly sensible to ask whether they are restrictive, or even adjectival. The noun is clearly used, being brought into a 'descriptive' (as opposed to a 'possessive') relationship with the head noun.

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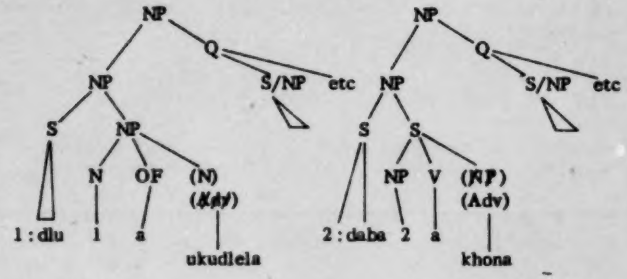

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(d) The 'have construction. Tazikala identifies many of the adjectives in Kijungan as falling into this category. The situation is slightly more difficult in this case, in that it is apparent that the basic element "it be with N/Adv" serves roughly as does the possessive, that is, to bring two nouns into a possessive-type relationship. However, this basic construction is brought as a whole into Relative relationship by use of a further possessive/qualificative; that is, if the Relative is, as Hagège claims, a verb element, then the language treats the construction as a whole as a verb. This seems possible, in particular seeing that I found it necessary to use a rewrite rule V→S (see 9.21 - 23 above). Doke, writing in 1927, found it necessary to comment that the whole construction was in the process of formation and that different speakers used it in different ways (see 9.14/15).

In general therefore, we can say that Zulu draws on most of the major parts of speech, in various different ways, for purposes of nominal modification.

.13 In the above I have not dealt with the enumerative (which has a slightly different concord but is clearly related to the Relative), negatives, (which in Zulu are a whole field on their own), or with operators (particularly "all" and "each", which are also differently concorded, though related to the Relative also), nor with the first and second person, (which follow the lines drawn above except for minor details); as these are outside the scope of this paper.

How do Zulu & English compare?

In detail there are the expected differences between the languages. Zulu shows some evidence (at least according to Venneman's (1972) diagnosis) of being an SOV language in origin - vide the preposing of the objectival concord. But there are at the same time some quite striking similarities in their respective approaches to nominal modification. Both seem to use the paradigm deep categories of argument and predicate as nominal modifiers. The striking 'independence' of the complete adjective in Zulu (stem + concord) and of the complete Relative are unexpectedly paralleled by the evidence provided by Ross' tests as to the 'sententiality' of English adjectives. Between the assignment to underlying functional category in the two languages, a similar hesitation, or 'squish', seems to be evident - in particular in English in the case of referent modifiers and in Zulu in the case of Relatives, many of which have nominal stems but all of which are treated as part of the verb system. It is also claimed for African languages (e.g. Takizala (1974))

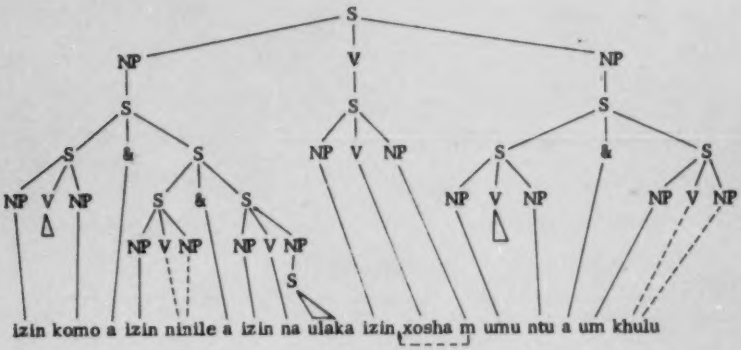
that their system of nominal modification is bound up with the use of the possessive. This I have shown to be the case, but I have attempted to point out that this varies considerably in detail between one construction and another; thus we find in Zulu a construction resembling quite closely the derivation posited by Levi (1973) for 'criminal lawyer', equally employing noun-based modifiers in the construction. We also find a construction very similar to that pointed to by Hudson (1975) for -ed adjectives of inalienable possession. Interestingly, this construction extends beyond simply inalienable possession to the domain of other nominal elements also.

But most interestingly of all, if we examine the Relative construction in Zulu, its closeness to the English system of referent modifiers is remarkable. While much work has been done on relating various English verbs and particles to one another, little if any has been done on African languages; but the preliminary investigation done above does seem to indicate that the same state of affairs holds good for Zulu and in almost exactly the same areas. It is too soon to start proclaiming the confirmation of universal relations between semantic primitives, but perhaps some arrows have been pointed in that direction. In particular, it would be interesting to know if any further evidence were to be adduced regarding the similarity of the particles in Zulu, or regarding the nasal syllable and the primitive 'BE'; or regarding the semantic composition of items in English like 'who', 'which', 'that', etc. If it were to be confirmed that any syntactic facts could be explained in terms of the decomposition of this element into 'pronoun + particle', I for one would find it very interesting! As it is, there is clear case marking in the (dying) form 'whom'. If 'who' were to decompose into 'of he', for example, then the correspondence between Zulu and English would be fairly remarkably close.

In conclusion, it is interesting to note the correspondence between the Zulu use of adverbs to construct 'tense' modifiers and the close relation in English between adverbs and 'tense' modifiers. In Zulu too there are definite differences between these elements and the usual run of nominal modifiers.

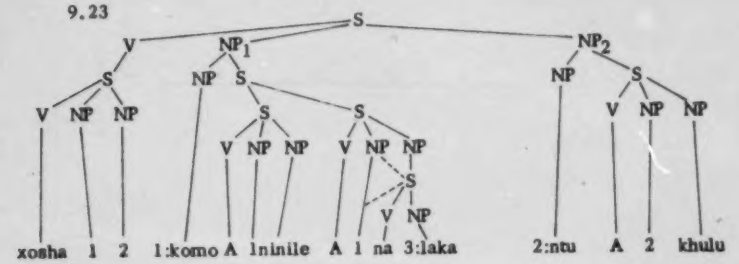
I have commented on the very close relation in form of the verb and Relative, so this may be feasible. Note that the problem is greater in concords without nasal syllables! Again, evidence for the Noun class prefix being as it were a self-sufficient pronoun is slight - a problem when we consider that the standard Transformational treatment of the pronoun is to replace it in deep structure by the noun it represents - clearly an unacceptable situation in this case. A slightly better alternative might be:-

9.22



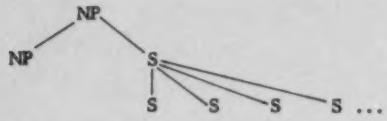
This still requires full sentence status for noun, verb, etc., but 're-allocates' "a" to the status 'and', 'with', which is in fact the province of "na". * If, on the other hand, I were to use McCawley (1971) and his system of co-referential indices, I could treat 'a' and 'na' as related, as verbal operators, but not necessarily identical. Thus:-

9.23

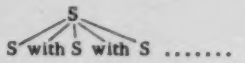


* 'na' is marked as a verb according to the structure in 9.22.

where the indices serve doubly as concords and relation markers. Again however there are unsatisfactory features. Note that the re-ordering transformation would have to make a series of exceptions for relatives in 'A', where seemingly by accident the ordering is right as above. The structure as above also correlates the Relative with the adjective, and therefore provides no structural explanation as to why the concords for the two should be different. However, the form of the embedding of relatives is rather satisfactory; the structure:-

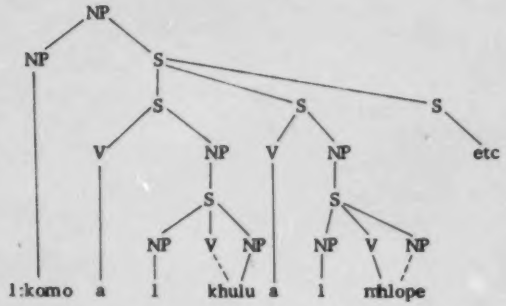


is very satisfactory for Zulu, especially considering the totally variable order discussed above. Perhaps a structure along these lines but conjoined by the prepositional element 'with' best serves to describe the use of the qualificative in Zulu:-

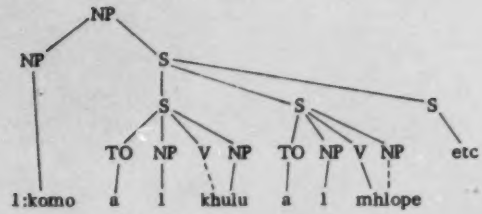


This element cannot be 'and' - see example 6.3 above. Unfortunately, 'na' serves as 'and', 'have' and 'with' - 'a' is 'of', 'to', as might be exemplified by "to NP - quality x". Some possible revisions are therefore:-

9.24



9.25



supposing for the moment that the difference between Relative and adjective were to be assigned to a difference in deep-structure part-of-speech classification. The element which would control the structural embedding would be the co-referentiality index; if they did not correspond between head NP and subordinate S's, the embedding would be blocked.

Note that different forms of the concord are used, resulting in different structural consequences, for such 'relative clauses' as are represented by the English translation "the man who I saw", etc. - the indirect relative. This fact excludes the above structure from having directly to correspond to such structures as would result from this use of the verb. There is, however, no space for an interesting digression of this sort here!

.10 There is a very clear unsolved problem which bedevils the analyses of section .9 above, which becomes clear when we consider the steps to be followed, presuming on the basis of reasonably interesting evidence above that the predicative use of the respective stems is primary, to derive the embedded forms from the predicative forms. I have presumed on the basis of some evidence above, notably

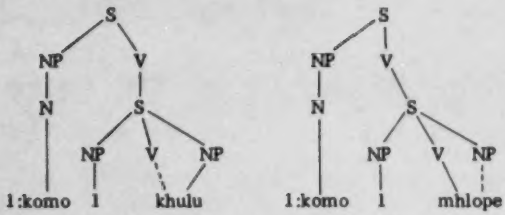
(i) Simplicity of rule formulation for the adjective;

(ii) Clear evidence of the affixing of the qualificative 'a' to a fuller underlying concord form, both for the Relative and adjective concord formatives;

(iii) The apparent self-sufficiency of the Relative and adjective in predicative form, and the clear subordinacy of both in the 'attributive' form;

that the predicative, sans qualificative particle, is closer to the underlying form. This provides predicate structures of the form:-

10.1



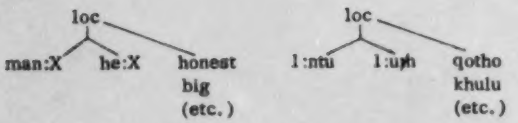
again presuming deep part-of-speech differentiation.

Clearly, when these are embedded in a structure of the form 9.24/25, even if we presume the embedding to be an operation controlled by co-referentiality index, there is no reasonable explanation for the appearance of the qualificative particle in the embedding, either as per 9.24 or 9.25 above. Clearly one could specify a transformational operation which would simply insert such a particle, or one could ignore the clear non-pronominal character of the particle and insert it as a deletion marker in the same way as English structure will accommodate the pronoun rendered as 'who', in a structure very similar to 9.25. However, the first operation is certainly non-explanatory, and the second, given the clear evidence of the form of the Zulu possessive, as in 9.1/2 above, for example, shows the second to be incorrect - though incidentally there is quite a good case for reconsidering the exact form of the pronoun 'who' and its relation to such semantic elements as 'and', 'of', 'have', etc.

There are good grounds for considering the predicative in Zulu as primary. However, let us overlook such grounds, and examine in particular the consequences of postulating the qualificative as present in the deep structure of the Zulu predicative.

In such case, the particle clearly appears to assume the status of a case marker - a semantic-syntactic role expressive of the relation between head noun and predicate; ie. as above, NP_X(to NP_X - quality Q), where "to" clearly expresses some form of locative arrangement. This would make the utterance of the predicate, e.g. "zimhlope", of the order of an utterance, "(it's) in the yard", where the reference (at least) has to be pre-established for the utterance to be felicitous. I say "utterance" advisedly, in that the syntactical status of such a string is less in question! To borrow some localist structure:-

10.2



which unhappily fails to differentiate Relative and adjective in structure. It represents the claim that the qualificative "a" is present in the predicative, possibly to be deleted by some minor rule later in the derivation. (e.g. the initial syllable elision rule 8.9). This would then explain why it appeared in the attributive.

I find this argument a little unattractive for the reasons 10(i) - (iii) above. Also, it is very important that the difference between Relative and adjective should be accounted for. Ross' Principle of Semantic Relevance specifies that all consistent recoverable deletions are likely to be of semantic importance; and if we wish to express the difference between two semantic situations by providing a separate explanatory structure, a structure which fails to provide such an explanation for the deletion is clearly unsatisfactory. In this case the structure provides no insight into the deletion of the nasal syllable from the Relative, whereas previous structures provided room for the differentiation.

It is also important to consider the (surface) difference that the presence or absence of the particle makes.

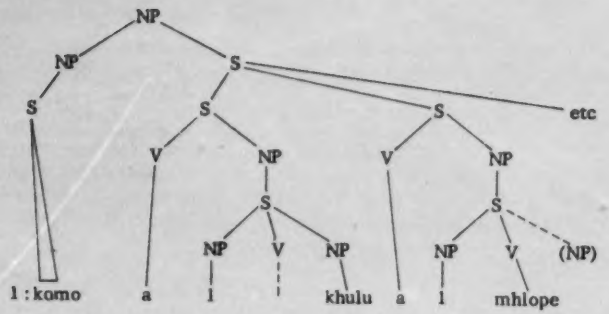
9.17 omkhulu ufikile
"to him is bigness, he has arrived"

9.9 zinkhulu
"they are big"

10.3 ezinkhulu
"they are the big ones"

In 9.17, 'omkhulu' has readily-perceivable pronominal status, (as in "who repents, shall be saved"). In 10.3 it has clear sentential status, with a pronominal flavour. But 9.9 also has sentential status. Any structure which adequately characterises the situation will have to reflect all of these facts. The closest to doing so so far is structure 9.24, here revised:-

10.4



This has several advantages:-

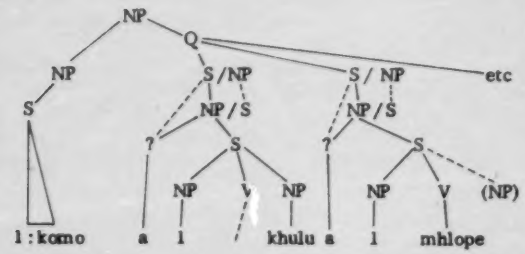
it records the sentence status of the predicate (as 9.9);
it records the sentential status of the attributive;
it indicates an embedding procedure;
it maintains the "affiliative" structure of 9.23 - 25;
it differentiates adjective and Relative structurally.

It also has several failings:-

it promotes the particle to the status of a verb, which is convenient for linking it to na, but inconvenient in trying to formulate an acceptable embedding procedure;
it does not point explicitly to the pronominal status of the attributive;
it requires a new rewrite rule, S → V NP.

One possible (if tentative) revision might be:-

10.5



This demotes the particle (to a case marker?) which, as above, has pro's and cons of its own; it clarifies on the pronominal status of the attributive; and it also requires a new rewrite rule, S – →NP.

Note that if we were to regard the English "who" as possessing some kind of subordination marker, of the form "who = to he, of he, have he", rather than as "who = head NP", then the same problem arises. At present I have no further suggestions for its solution.

.11 I have considered as nominal modifiers the Relative and the adjective, and (briefly) the "na" and the possessive constructions. There are some points still to be made concerning the latter two.

I discussed the "na" construction in 9.3 and 9.4, and in 9.19 - 23 above; to repeat:-

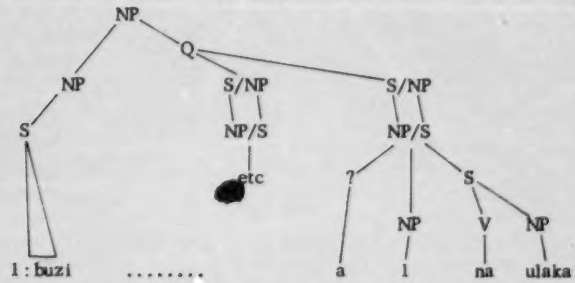
9.3 izimbuzi ɣzi +na +ulaka
"goats they have anger" / "the goats are angry"

It is to be noticed that embedding of this form runs into precisely the same difficulties as do the Relative/adjective. The attributive form, "angry goats", is:-

9.4 izimbuzi a +izi +na +ulaka

This can be embedded equally well into the structures of 10.4 or 10.5:-

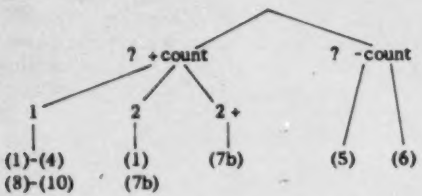
11.1



However, a number of forms in the direct possessive exist which exhibit properties of nominal modification, but which present different embedding problems. Consider:-

seems to be that it includes rope and veins. But there is a 'coil of rope' in (1). As for (7b) the set-like basis seems to be clear, but what could be the reason for a pair of shoes to appear here, while two dollars, (presumably identical) appears in (1)? We notice too that (9) specifies single objects. Is there then a subsystem of number based on the widespread 1, 2, more than 2 pattern? We might have a hierarchy like this superimposed on Fig. 2:

Fig. 4

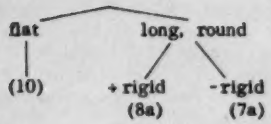


Going back to (7a), are we justified in calling this category 'long, non-rigid', since it contains arrows - an object conspicuously useless unless rigid? Or is the 'sameness' of the arrows sufficient to assign them to (7b)?

These are not questions that can be answered from this side of the Atlantic. The only practical solution is to take the glosses provided by the source at face value, even if it is sometimes not clear from the examples given how accurately they match up with the denotata. In the present instance this has the pleasant effect of making the shape hierarchy appear neater and more regular than I suspect it to be in actuality.

Still another problem which emerges is concerned with the exact interpretation of the features themselves. For example, if a language is reported as having a 'round' category, and this includes items like stones and eggs, do we conclude that it should be in fact 'round x round' - a three dimensional category which in some languages might be contrasted with 'round' (say, bead vs. coin), or are we justified in simply leaving it at 'round' on the assumption that some less differentiated Gestalt like property is at issue? Depending on what decision we make, we might want to leave Fig. 3 as above, or include the combination of round and long present in some of the Asian examples quoted earlier:

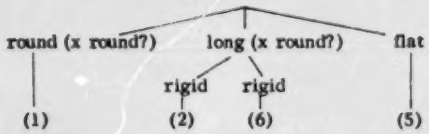
Fig. 5



Where a decision is clear I have made as fine a distinction as possible. Either way, these features seem to me to be more useful than the 1, 2, or 3 dimension distinction which has occasionally been used in this area. It is difficult to see how the principled application of such abstract features could exclude anything we talk about in the real world.

Hupa (Haas 1967:360), Pacific Coast Athapaskan, NaDene phylum has a system of verb stems which classify. Its categories are described as 1) round objects 2) long or sticklike objects 3) living being (animal or human) 4) container with contents 5) fabric-like objects 6) several objects, or ropelike objects 7) granular mass 8) dough-like or mudlike objects 9) piled up fabric (blanket). Unfortunately, no more detail is offered in Haas's account. However, the data appear consonant with our working hypothesis. In terms of the features used here, we can represent the categories of shape, interpreting 5) as 'flat', along the same lines as the Chipewyan example as follows. (The similarities between the two were noted by Carter):

Fig. 6

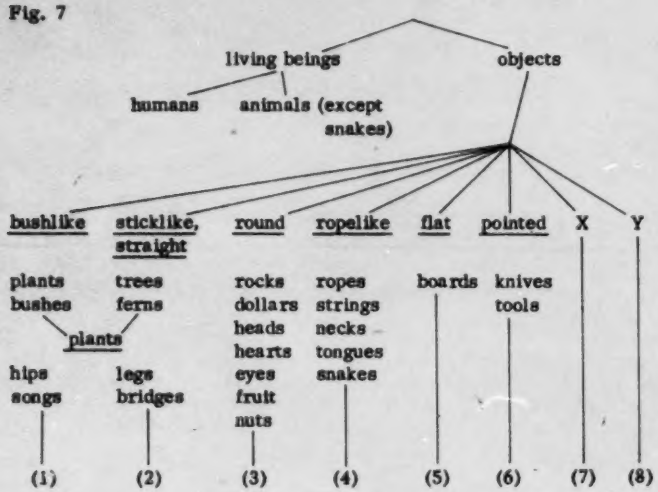


Notice that the 'container and contents' category appears again. It is not possible to tell, on the basis of Haas's account, how far it overlaps the more straightforward shape dimensions.

Haas also discusses Yurok, an isolate of the Algonquian-Wakashan phylum, which has a system of suffixes added to numerals and adjectives. It has many similarities with Hupa - whether its system is more complex or whether this is just a reflection of the greater detail in her description is not clear. The categories of the language are presumably entirely overt - an informant would have no difficulty in providing a gloss, since the same 'object' can be classified two different ways, producing a change in meaning. Thus /ci.sep?/ with the 'bushlike' classifier means 'flowerbush', with the 'round' classifier

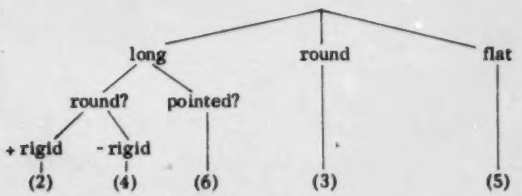
it means 'flower'. The hierarchy sketched by Haas, in its relevant points, is as follows:

Fig. 7



No. (7) contains almost everything which doesn't fit into any of the other categories; No. (8) is a special set of categories containing items of special or ritual importance, like houses, boats, and woodpecker scalps. This data could support the following arrangement, though perhaps the inclusion of (6) is dubious:

Fig. 8



The container category is absent, but the bushlike category which was reported for the Asian languages (gathered loose things) makes an appearance. As Yurok is an isolate it is not easy to tell how representative of its phylum it is, but the resemblance to Hupa and the Apachean languages is striking. Yurok is the only Algonquian-Wakash

phylum example I have found detailed data on, so far. However Nootka-Nitinat (Haas 1972) has a class of 'stem-extenders' which appear to classify partly along these lines:

1)a spherical protruding object
  b " " non-protruding object
2) mass of small objects
3) stiff, hard objects
4) objects standing upright, rigid
5) flat, flat against
6) broad flat surface (p. 86-87)

Thus the features round (x round) (1), rigid (3), long x vertical x rigid (4), and flat (5), flat x long (6) seem to appear. However, this assignment of them is not entirely straightforward, and Haas warns that "the meaning of stem extenders is at best extremely vague and little can be said about it at the present time" (p. 89).

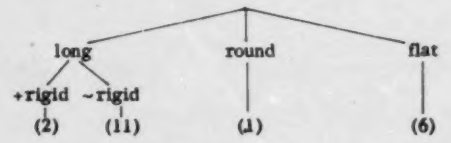
Returning to the Athapaskan group, the detailed analysis of Western Apache in Basso (1968) suggests that our implicit course, of regarding rigidity as a feature along the same dimension as length may be mistaken. It is interesting in this context to compare the complexities which emerge from Basso's analysis with the apparent neatness of an earlier treatment. Hoijer (1945) describes the verb stems of Apachean as:

1) round object
2) long object
3) living being
4) set of objects
5) rigid container and contents
6) fabric-like objects
7) bulky objects
8) set of parallel objects
9) a mass
10) wool-like mass
11) rope-like object
12) mud-like mass

((4) and (11) overlap in Chiricahua and Navaho: compare Chipewyan. Chiricahua includes 'more than 2 objects' in the same category)

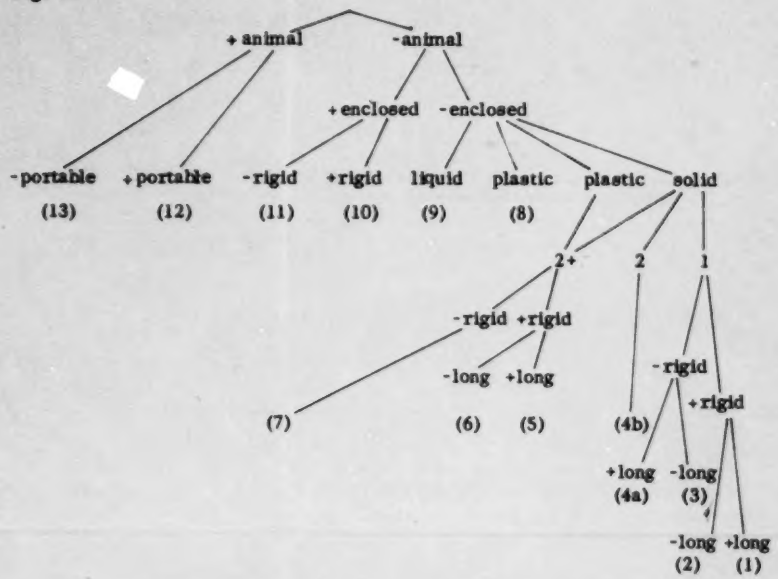
We might propose on the basis of this data a shape system something like:

Fig. 9



-which appears parallel in relevant respects to the predicted pattern. However, Basso systematises the entire classificatory verb system into the following ordering, based on a componential analysis similar to the one I have been using:

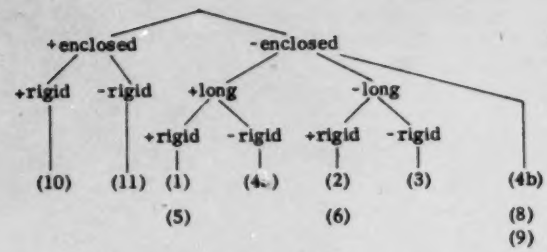
Fig. 10



Notice that this isn't, strictly speaking, a tree, since two branches converge; nor are the categories in total correspondence with those used by Hoijer.

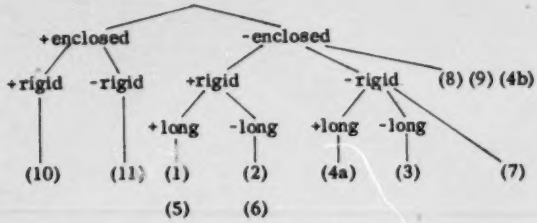
The problems arise when we try to filter out from this system the dimension of shape. Since there is a minimal discrimination of the feature ±rigid, between (1) and (4a), a diagram could be constructed:

Fig. 11



However, such an arrangement provides no way of fitting in category (7), since it predicts the absence of any category combining the features of ±rigid enclosed, without ±long. But (7) is just such a combination, -rigid, -enclosed, suggesting that we ought rather to have an arrangement like:

Fig. 12



And if this truly reflects the data, then we have here a counter-example to the hypothesis that rigidity is a feature secondary to length. It is a counterexample at least to the strong hypothesis - that ±rigid is subordinate to ±long - but is compatible with the weaker version that whenever ±rigid appears, so does ±long.

However, although I have been considering rigidity to be within the dimension of shape, as was implicit in the work of Adams and Conklin, it is by no means clear that this is correct; in fact there is, I would think, a good a priori case for rigidity belonging to a dimension of 'inherent quality' or something similar, perhaps along with features like 'liquid' or 'granular'. Thus the strong version of our hypothesis would have to be amended to say that rigidity is part of a subordinate dimension dependent upon the prior appearance of some element in the dimension of shape, such as length or roundness. This, an attractive and plausible hypothesis, is also clearly disconfirmed by Fig. 12.

Shape Classifiers and Natural Categories

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Occasional Papers. pp. 35 - 57

Many Asian, American and African languages have ways of classifying objects, either with a system of numeral classifiers, or with classificatory particles on nouns and verbs, or sometimes with basic verb stems. The similarity of the semantic content of these categories across languages, even when they are realised differently, has long been noted (Haas 1967). It seems reasonable to expect that along any particular dimension (e.g. texture, number, shape, function) certain characteristics will be more widespread than others, and that some kind of implicational relation may exist between them, perhaps universally, in the same way that, e.g. colour terms are known to do. It also seems reasonable to expect that the existence of such universals would reflect the saliency of those characteristics over others, though whether this saliency would be psychological or cultural would be a matter for further investigation. However, in the light of recent work on "natural categories", discussed below, it seems likely that at least for some dimensions, universality might be linked to psychological saliency, even if this is not reflected in evolution or acquisition.

Thus the position taken here is in opposition to that of some previous workers on systems of classifiers, who have claimed that such systems are based on essentially arbitrary collections of some or all real world objects. The numeral classifiers of Yurok, for example "impose an arbitrary classification on all objects in the 'real' world" (Haas 1967:359). If taken literally, this claim is self defeating, at least if accompanied by taxonomies labelled 'ropelike' etc. What is meant, I think, is that there appears to be no reason why this common element should be picked out for encoding rather than any other. I take the view that there is a reason, at least in some dimensions, in particular that of shape: the reason is that some shapes are more 'natural', more perceptually salient than others equally, or more likely to be encountered in the real world. This view, then is in opposition to the view that only culturally important shapes would be those chosen for encoding; both of these views are in opposition to the 'arbitrary' view.

Shape seems an area ripe for enquiry. Many languages have highly developed systems which classify objects according to their shape, and it has the advantage of being a dimension which is a little less ambiguous than others. Previous writers have claimed in rather vague terms that shape is a semantic dimension universally encoded, even in languages with no overt classifier system. (Friedrich 1970:380). This is quite plausible, but depends on what interpretation 'encoded' is to receive. If it means having a morphologically distinct particle, or particles then it is clearly falsified by many languages, including most of the Indo-European group. And if it only means something like 'expressible by a morphologically independent item', it is trivially true for pretty

nearly all languages (with the possible exception of Dani: discussed further below).

A first step is to discover whether there are universals of shape in those languages which mark it by a system such as those mentioned above. Some progress in this direction has been taken by Adams and Conklin (1973) who carried out a study of numeral classifiers in 37 Asian languages, which included representatives of Malayo-Polynesian, Austro-Asiatic, Mon Khmer, Sino-Tibetan, Altaic, Dravidian and Indo-Aryan language families, in order to "illustrate sets of implicationally ordered parameters by which nouns are classed for counting" (p. 1). As far as the category of shape is concerned their analysis reveals the following results: Primary features are long, round, and flat. Secondary features are rigid/flexible, relative size, empty/full, irregular/regular, part/whole, horizontal/vertical and edgedness, with the latter two applicable to the primary feature of length only. Secondary features are those which cannot themselves provide the basis for a system of classification. Notice that there are two ways of interpreting a statement such as ' "rigid" is a feature secondary to "length" ' :1) whenever ± rigid is used in a language, ±long is too, but not vice versa, or 2) whenever ±rigid appears, it is used to further discriminate along the ±long category. I'll refer to the first as the 'weak' hypothesis, the second as the 'strong'. The same thing applies, mutatis mutandis, to the other categories.

Primary features can combine with each other, (except that strictly speaking, a long round flat object is not geometrically possible - round would have to be interpreted as 'curved'), or with secondary features to achieve systems of considerable complexity, as in the following examples, taken from several different languages, (p. 6):

long (tree)	round (sun)	flat (mat)
x flexible (rope)	x round (sphere, ellipse egg)	(seems to combine with texture)
x flat (tablet, strip of wood)	x long (bone)	
x round x solid (bamboo)	x flat (coin)	
x 'gathered loose things' (broom)	x size - large (fruit) small (bead)	
x flat x thin (sieve, mattress)	x empty (ring)	
	etc.	

Several other secondary features are quite common, such as 'pointed ends'.

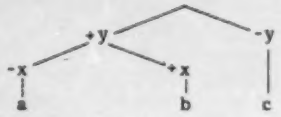
One of the conclusions reached by Adams and Conklin is that "One of the most fascinating facts of numeral classification is its dependence on the visual feature of form" (p. 8) and: "It is form and not such visual features as color which is salient" (fn.).

I have tried to carry out a similar, though restricted survey for the native languages of the North American continent, focussing on the dimension of shape. The working hypothesis was that such a survey would reveal an implicational hierarchy at least not distinct from that found by Adams and Conklin. Before presenting the results of this survey I want to mention some important problems of method, both theoretical and practical. Firstly, there is the perhaps inevitable problem that the survey is based on secondary sources, and rather limited sources at that. Secondly, the available work with the rare exceptions of the work of e.g. Berlin (1968) and Friedrich (1969a, b.) is not usually primarily interested in this rather specific area, so the relevant data has to be ferreted out from the various aspects of the discussion, and consequently, even where the data is relatively clear, the interpretation of it is not always easy or unambiguous. Thirdly, the assignment of an object to a category by a native speaker may very often be based on a metaphorical extension of the category, as for example, the frequent inclusion of "song" in the category "long", presumably generalising geometrical length to chronological length. This example is reasonably transparent - others are far less so, and therefore the content - i.e. the extension - of a category may be misleading as to its nature. It is in any case not clear that the sense of a particular category ought to be determined by abstracting that feature or features which its referents have in common, although this approach seems to be implicit in most of the work I've looked at. Few linguists until the fairly recent past placed any faith in the judgements as to the meaning of the categories offered by the informants themselves. But it is not easy to arrive at a definition even when the description of the extension of a category is detailed and clear. Basso (1968) contains a useful discussion of these problems: he describes two categories of Western Apache, one of which contains items like puppy, worm, moth, infant, the other heifer, steer, pig and human adult. He says: "The analysis of these two categories, more than any of the others, was influenced by the interpretation of my informants. It was they who brought to my attention the deceptively simple dimension of relative weight, and by extension, ease of portability. Prior to this, I had been unable to adduce any set of criteria which effectively discriminated among the members..." (261).

A fourth problem is that classifier systems, however they are realised, always classify along several different dimensions, which do not always form a neat and tidy tree structure - there is overlap and cross

classification. This makes it very difficult to separate out the sub-system of shape which I am interested in. And although the normal response to cross-classification is to introduce some kind of distinctive feature notation, it is not clear that this doesn't beg the question as to whether the separate dimensions should be treated as alike in kind, even if they can be. In practice what I have done is to approach the data with an a priori (not ad hoc) set of features based on the findings of Adams and Conklin. Where the data is compatible with an assignment of these features I have assigned them on the following principle: when two classifiers differ minimally by one feature, (as determined either from the description of the senses of the classifiers, or from the description of their referents), then that feature is subordinate to the ones they share. Thus if classifier a differs from classifier b in that it lacks feature x which is present in b, but a and b themselves share feature y which distinguishes them from classifier c, then the following tree can be constructed:

Fig. 1

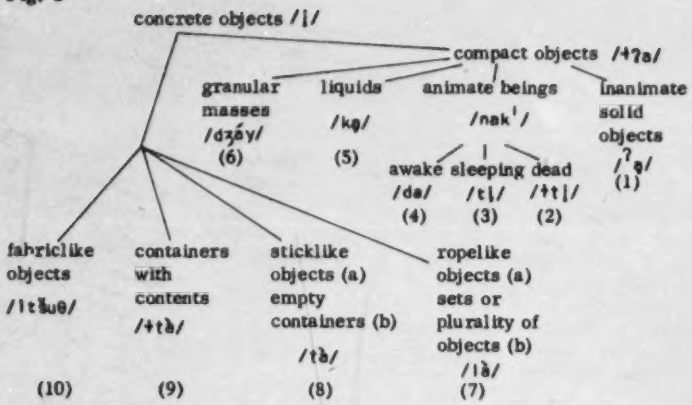


and feature x will be said to be subordinate to feature y in this system. Notice that there is no requirement that every feature has to be morphologically marked. Thus I do not distinguish between 'overt' and 'covert' categories within the domain of shape. (See Whorf 1956:91 for discussion of this distinction with respect to shape).

As an illustration of some of the difficulties which can arise I'll work through one analysis in some detail. The findings of the wider survey can perhaps then be judged according to the merits or otherwise of this example.

Chipewyan, a Northern Athapaskan language, NaDene phylum, has a system of classificatory verbs which assign to their noun "number, shape, texture, purpose" (Carter 1976). The hierarchy so formed can be represented as in Fig. 2 (from Carter):

Fig. 2


concrete objects /ɬ/
compact objects /ɬʔə/
granular masses /dzə́y/ (6)
liquids /kǫ/ (5)
animate beings /nak'/
inanimate solid objects /ʔǫ/ (1)
awake /də/ (4)
sleeping /tɬ/ (3)
dead /ɬtɬ/ (2)
fabriclike objects /ɬtšuθ/ (10)
containers with contents /ɬtə̀/ (9)
sticklike objects (a) empty containers (b) /tə̀/ (8)
ropelike objects (a) sets or plurality of objects (b) /lə̀/ (7)

Some examples of the items 'denoted' by each classifier are:

(1) lake axe dollar coil of rope coin knife hat sewing machine tongue kidney hide button ground skeleton ice two dollars pipe stove ear ribs back claws anus, etc.
(2) dead person or dog, raw fish.
(3) person, sleeping baby or bear, girl.
(4) frog spider sitting person or bear, beaver.
(5) mud (in or out of container) blood water tea milk.
(6) pile of sand or sugar or instant milk, fish eggs, tobacco.
(7) arrows glasses rope more than one fish, coin, dog, etc., veins pair of shoes.
(8) bow empty box canoe cigarette firewood toboggan coffee cup penis frying pan.
(9) full box single can of beer, coke, etc., pack of cigarettes single bottles and containers full of coffee, whisky, etc.
(10) calendar pants sheet of paper bear hide, book, one glove, leaf blanket, grease.

Carter mentions that there are other classifiers such as /tšu/ "for a purpose", which can override many of those above. He presents an 'algorithm' to describe what the speaker of Chipewyan must decide about the categorisation of the objects he is talking about: (adapted from p. 30).

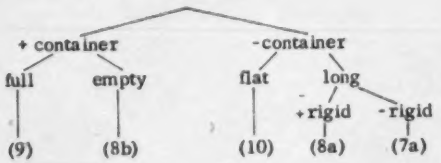
(1) If the situation doesn't involve a concrete object, FINISH.
(2) If consumption or intended use of object is involved, use /tšu/ and go to FIN.

(3) If object is in a container assign it to (9) (Fig. 2) and FIN. If not:
(4) If part of a set or plurality of objects use (7). If not:
(5) Assign all other objects as in Fig. 2. Any concrete object not specified assign to (1). Then:
(6) Assign verb stems in accordance with syntax, morphology, etc. FINISH.

It is not clear whether the 'concrete object' and 'compact' classifiers perform the function suggested by Fig. 2 or whether they are convenient glosses suggested by informants.

How can we abstract those categories operating along the parameter of shape? As a first approximation we might suggest that (10) glossed as 'fabric-like' could with equal justice be glossed as 'flat' and perhaps 'non-rigid' (except for book and (?) calendar). The last item 'grease' seems an exception to both glosses, unless it refers to an actual layer of animal fat. This is not clear from the context. Next, (8)b and (9) look as if they might support a full/empty distinction on the super-ordinate feature 'container'. Carter suggests in fact that (9) might derive from (8), since there is a form /θɛttə/ which can be translated as either 'a container with contents lies there' or 'he is sitting in a canoe'. However, for some reason "mud in a container" gets classified as (5) not (9). Next, we might try to establish a 'long' category. (8)a objects seem to support this, and they are all 'rigid'. (7)a, glossed as 'ropelike' by Carter, suggests that we might have a 'long, non-rigid' category. Thus ignoring the plurality aspect of (7)b, we might set up the following abbreviated hierarchy:

Fig. 3



Apart from the new feature ± container, this hierarchy is consistent with our working hypothesis. The new feature is quite widespread in Amerindian languages, and appears to play a part in some African languages too, so is independently needed.

But when we compare the denotata of the categories with the glosses provided by Carter, this pattern does not look quite so tidy. First of all, it is difficult to see in what sense the objects in (7) - (10) are any less 'compact' than (1) - (6): in fact some articles in (1) are also found in the higher numbered lists. We've already noted the exception to (9) in (5): on closer inspection, the only justification for the gloss of (7a)

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What Might a Universal Theory of Cases Look Like?

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1. In this paper I want to talk about the relationship between certain aspects of morphological form and the syntax and semantics of what are commonly known as 'case relationships'. There are four distinct concepts commonly associated with the word 'case' which are significant in the context of the present discussion:

i. 'Case forms' - the formal expression of the relationship between a noun and some other element in a sentence; case forms may be realised in many different ways, including bound affixes, prepositions, postpositions, serial verbs, etc.

ii. 'Case affixes' - bound morphemes attached to the end of nouns; these are a subclass of (i), and will not be given much special attention in this paper.[1]

iii. 'Case uses' - pretheoretical concepts of identical relationships; loosely speaking, relationships which are semantically 'the same'. This cannot be anything more than an intuitive concept, and case uses may be established on a more or less generalised basis - e.g. 'be in the box' and 'be in the room' could either be considered examples of the same case use, or be differentiated as separate case uses. In this paper, a fairly intuitive and generalised notion of case use will be adopted, largely for practical reasons.

iv. 'Deep case' - elements of a particular theoretical framework ('Case grammar') which differ from case uses in that they generalise not on the basis of intuitive similarity (hopefully), but on the basis of such factors as distribution, morphological realisation, and other formal means by which linguistic elements are established.

Given the definitions above, this paper sets out to examine the relationships which case forms and case uses have with each other in a number of languages, and to examine the notion of 'deep case' in the light of the results of this investigation. I shall claim that the facts which appear to emerge demand explanations which are beyond the reach of most current theories in linguistics, and shall argue for a semantically-based model of a very specific sort, although without any real conviction that this is the right answer.

2. If one is searching for universally valid statements about case uses and forms in language, a number of plausible candidates suggest themselves, some of which have already been suggested in the literature. For example, one might claim that the subject and object of

[1] One reason for mentioning them, however, is that they are frequently treated as having some special status among case forms (in the sense of [i]), cf. Jakobson 1936.

a transitive verb must be distinguished formally (cf. Comrie 1975). This sort of claim is based on plausible assumptions about the necessity for the avoidance of ambiguity in basic sentence structures. It depends on at least two factors in addition to this: (i) on the universality of the notions 'subject' and 'object' (a position which receives much positive, but some negative evidence in the papers in Li (ed) 1976), and (ii) on the independence of these notions from semantic factors. Both of these prerequisites are supported by evidence of various sorts, but neither can be said to be established as yet.

A further type of proposal that one might make would be a hypothetical claim of the following sort: given a distinction between locative, allative and ablative relations, there is a relationship between them of a hierarchical nature such that a distinction on one level between positional parameters (such as 'inside' or 'top') implies that the same distinction is made at a higher level in the hierarchy, but not vice versa. Thus in Russian, for instance, there is a distinction between 'over' and 'under' in the locative, but not in the allative or ablative (at least, not without lengthy paraphrase). This particular claim is (to my knowledge) untested, but if it turns out to hold, it will seem entirely natural. This position can in turn be complemented by a hierarchy of parameters: if a language has a set of case forms meaning 'at under', 'to under', 'from under', then we would expect it to have a set of case forms meaning 'at inside', 'to inside', 'from inside', but possibly not vice versa.

Yet another type of universal statement that one might wish to make could involve universal ways in which case uses may be generalised. Such universals are in practise assumed in any linguistic analysis (and also commonly in such activities as language teaching); 'in front of the house', 'in front of the tree' and 'in front of the lake' are likely to be similarly expressed in all languages (another untested claim), although there is no logical necessity that this should be so. A more contentious claim of the same type would be that the relationships 'fill with' and 'cover with' are identical (in some sense) universally, although they are clearly distinguishable, and in fact seem to correlate with the locative notions 'in' and 'on' respectively. Such hypothesised 'universals' are readily testable, and, if true, of considerable significance: there is, however, a sense in which the truth of any of these claims would not be surprising to the linguist. There are a number of proposals in recent linguistic literature which would accommodate such facts nicely. To take just the last claim, for example, the with in English corresponds to a nominative case (or objective, following Fillmore) in a case grammar description, and their identity could be 'explained' by this fact.[2]

3. In addition to the sorts of universals described in § 2, it appears

to be possible to find other types which do not fit in with recent linguistic theories in the sense that these theories would be forced to describe them arbitrarily, and therefore fail to account for the fact that they occur so widely. Stanley Starosta (1973) has described a number of such facts on the basis of a small number of languages. For example, the expression of instrumentality, manner and comitative is frequently identical in languages, instrumental and agent, as well as instrumental and objective (in the use described in § 2), are frequently expressed by homonymous case forms. As Starosta says (p. 2):

> "If we state that, say, a preposition that realises two different case relationships is really two homonymous prepositions, then there would be no way of showing at the syntactic level the generality that certain case relations tend to be closely associated with each other in a large variety of languages".

Starosta's proposal for incorporating this generality into linguistic theory is that there is a universal set of case relationships ('deep cases' in the sense of § 1) and also a universal set of case forms (the traditional nominative, accusative, dative, etc.), and that the mapping between these two sets of elements is to some degree universal. An extended quote will perhaps best describe the approach:

> "When we see the same patterns emerging in geographically remote and genetically unrelated languages, it becomes impossible to believe that these patterns are only accidentally similar. The same surface case manifestations or case forms, identifiable by comparable syntactic criteria in various languages, realise the same groups of case relations. To describe this phenomenon properly, it becomes necessary to assign labels to case forms, and it should not come as a surprise that these labels can be conveniently drawn from the list of overt cases used in traditional syntactic description. It is necessary for the grammar to express regularities in the mapping of case relations onto case forms in order to achieve explanatory adequacy. Since case relations constitute a universal set, and since they can be considered to be subdivided into natural classes on the basis of choosing nondistinct surface manifestations in unrelated languages, it is reasonable to assume that these surface manifestations or case forms may also constitute a universal set. Once this assumption has been made,

[2] There are problems with this analysis, and more conditions would need to be put on the correlation of with and the nominative (terminology of J. M. Anderson [1971]), but this does not affect the point I am making that the identity seems a fairly natural one from this point of view.

it becomes possible to state formally the systematic correlation between relations and forms where it occurs, and to indicate which form-relation associations are universal". (Starosta p. 3).

Unfortunately, Starosta's proposal leaks in a number of places, and cannot in my opinion qualify as an explanatory theory of the relationships between case forms and case uses. One would expect a theory of this sort to have problems with data: but my contention is that it is also conceptually incoherent.

In many ways, Starosta's distinction of case forms and case relationships (like the distinctions I have made in § 1) is purely a framework in which it can be stated that correlations exist, rather than an attempt to say why they exist. In some of these associations it is possible to detect common semantic features (e.g. in Starosta's deep cases DAT(ive), LOC(ative) and DIR(ectional), where some notion of location is clearly present[3]). But other associations, such as that of the Comitative and Objective, are characterised by Starosta as 'arbitrary', and evoke the somewhat desperate comment: "All a grammarian can do in such situations is state these associations, and ultimately hope for some sort of psycholinguistic or grammatical confirmation and justification for them". (p. 12).[4] It is, of course, possible for any linguistic theory to incorporate such ad hoc universal statements of identity between elements, but a large proportion of the data brought up by Starosta can only be considered as interesting material which demands some sort of theoretical treatment to 'explain' it, or at least to incorporate it into the normal descriptive apparatus of a theory. This Starosta does not do.

There are a number of compelling reasons why Starosta must be considered to be on the wrong lines in his approach to cases. Firstly, he accepts Fillmore's model of case grammar, and he is therefore open to the arguments that have been advanced against this model (including many by Fillmore himself [1971]); these include such considerations as the arbitrariness of the particular cases chosen as representing underlying relations as well as the virtual absence of well-substantiated

3 More constrained deep case models do not distinguish these deep cases (cf. Anderson 1971).

4 Less cautious grammarians (e.g. Nilsen 1973) claim that there are common semantic features in such cases also; e.g. Nilsen (p. 71) unites Instrumental and Comitative under the general heading Associative, a procedure which throws no light at all on how they might really resemble each other. It is difficult to imagine any case which could not plausibly be said to express association.

consist of at least one root morpheme with a 'basic' lexical meaning, combined with various stem forming morphemes which modify the semantic content of the basic forms. This combination results in the lexical inventory of the language as far as the verbs are concerned (Langdon 1970:80-89). Various plurality morphemes also combine. There are a set of prefix stems, among which are included the following meanings:

1) action denoted by the root is performed on or by means of one long object (includes large, hard, sharp objects such as knives, and people) (Langdon's (a))
2) action on or by bunch of small objects, or soft pliable objects, or liquids (=(b))
3) (rare) distorted object (action by, or result of action) (=(g))
4) soft, pliable objects (=(l))
5) action on or by small solid round object (=(s))

Again the flexible category (pliable) appears to be independent of the long and round categories, though these are actually manifested, unlike Acoma. The prefix described by 3) is the only example of a 'distorted' or 'irregular' category I have come across in isolation. I take this to be particularly interesting since in most non-industrial environments this shape-feature is surely likely to be more characteristic than the regular geometric forms, and so, one might think, more likely to be the sort of thing that a language would encode. We would certainly expect it to be more frequently encoded than it is, even granting some sort of priority to the regular forms.

Tzeltal (Mayan, Mexico) has been the subject of an exhaustive study by Berlin (1968). It has a classifier system of incredible complexity, using a bewildering number of cross-cutting dimensions. Thus any diagram is bound to give a misleading impression of the system. However, the following hierarchies emerge from a consideration of Berlin's semantic domain 11 (87-93):

Fig. 13

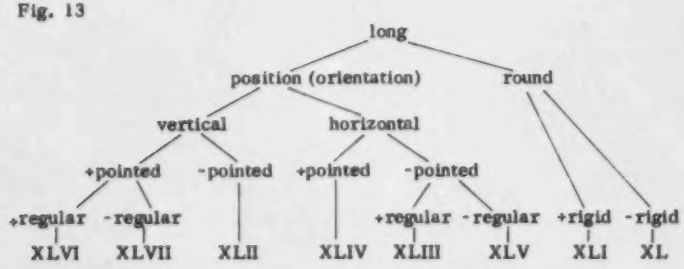
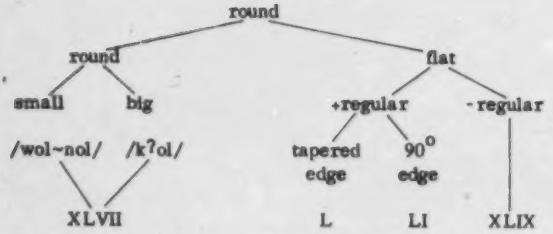


Fig. 14



(Roman numerals refer to the plates in the body of Berlin's text)

There are many other domains which may be partly ordered by the same features, for example, a class of long thin flexible objects coiled and tied in various ways, a class of long thin rigid objects variously warped, etc. Thus Figs. 13 and 14, though consistent with the data, are artificial in that they are the incomplete product of a prior arrangement of the linguistic material. It might be apposite here to quote Berlin himself: "It appears at this moment that the vast majority of classifiers combine into a large number of few-membered paradigm-like sets, or semantic domains, at the most specific level of generalisation. The relationships that hold between these sets do not appear to be taxonomic, at least in terms of a deep hierarchical ordering...(174)". "It may..be the case that neatly partitioned, multi-levelled taxonomic structures are not so much a principle inherent in 'human thinking' as they are artifacts of the special semantically circumscribed kinds of data that have been subjected to analysis (180)".

With this proviso, Tzeltal appears to be in accord with our original predictions, though the relevance of Berlin's comments to our other data should not be overlooked. Another Mayan language, Chontal, also has a richly developed system of numeral classification (Keller 1955) which appears to be similar in the relevant respects to Tzeltal, as indeed does Tzotsil - a language related to Tzeltal. It appears that this group of languages may be as similar to each other in this respect as are the Athapaskan (NaDene) group.

Finally, Tarascan, a genetically unaffiliated language spoken in Mexico, manifests all three of the types of classifier system found individually in other languages. That is, it has numeral classifiers, classificatory verbs, and body part suffixes. (I have said nothing about the latter category: however, they too are a widespread phenomenon which deserve a full study of their own). Friedrich (1970, 1969a, b) has described all the systems and made some general observations as to the frequency of encodings of shape. For details of the Tarascan

system it is sufficient to cite his papers: the general import is suggested by the following extracts: "three statistically frequent morphemes... imply classes of non-rational or pre-rational things with shape, usually visual shape... Both Tarascan Indians and Amerind grammarians tend to label such shapes as "longish, flattish and roundish" (1970:381). Friedrich unfortunately prefers to call these numeral classifier distinctions saliency of one, two, or three dimensions. The dubious information content of these labels has already been commented on. As well as this system "a considerable number of verbal roots are marked covertly for various features, notably smallness and the three dimensions longish flattish and roundish" (386). Intersecting with both these classes are the body part suffixes, the subject of his full length study (1971). Again, it is sufficient to cite this work, which reveals considerable complexity.

It is clear that Tarascan is not inconsistent with our original hypothesis. We can now summarise the results of this all too brief and limited survey: of the seven phyla (?) of North and Central America (Mexico), only two appeared not to have any languages at all without some evidence of shape classifiers. These were Eskimo-Aleut, and Aztec-Tanoan. It is probable that the first really doesn't have classifiers and that the second does, but I didn't find them. For instance, Papago is reported to have particles referring to whether is in a container with a broad or a narrow neck. We would predict that if such a fine distinction is made, the grosser ones should be there too. I have not been able to check this. The North American branch of one phylum - Penutian - seemed to have no languages with classifier systems, but of course I couldn't check them all. The Central American branch of this phylum however, included the Mayan family, in which such systems are widespread (Chontal, Tzeltal, etc.). Of the remaining four, the phylum with the most number of classifying languages appears to be the NaDene, which includes the Athapaskan group. Those languages of the Algonquian-Wakash phylum which classify do so along lines similar in many respects to the Athapaskan languages. A few members of the Hoka-Siouan phylum appear to have relatively impoverished systems at least one of which directly refuted our hypothesis that the feature of rigidity and its corresponding dimension only ever appeared either subordinate to (strong) or with (weak) a feature within the dimension of shape. The inclusion of Tarascan within the remaining phylum seems to be largely a matter of convenience so perhaps shows nothing about it.

The lack of direct genetic relationships between most of the samples examined make it impossible to invoke a 'common source' explanation of such similarities as were observed. And taken together with the results obtained by Adams and Conklin for the Asian languages, I think the following conclusions have suggestive, if not absolutely final support.

(Given the difficulties concerning the amassing and the interpretation of data which I have tried to stress throughout, it is difficult to see what would be final support).

1) If a language classifies along the dimension of shape, then it will have categories composed of the features ±long, ±round, or ±flat, or some combination of these.

2) If a language classifies along the dimension of shape, it is overwhelmingly likely that it will also classify along the dimension of which rigidity is a feature, and it is also likely that use will be made of the feature enclosed (though more needs to be said about this).

Other results of Adams and Conklin stand, as far as I can see, though I have not been systematic about other dimensions than that of shape. The question to be answered now is what do these cross-language convergences demonstrate?

As was discussed earlier on, there appear to be two hypotheses other than the 'arbitrary' claim which could be advanced to explain the observed consistencies. The first is that they are a reflection of cultural or social salience: that objects which are saliently round long or flat are objects of especial usefulness or significance in the cultures where these languages are spoken. This is an attractive and by no means implausible hypothesis, and I am convinced of its truth for at least some other domains. However, it is difficult to see how we could set about testing this, and in any case comments made by the investigators whose work I have been using do not indicate that there is anything especially culturally significant about these forms as opposed to other easily imaginable ones. The second hypothesis, on the other hand, which claims that the frequency of these features is a reflection of psychological saliency, in some sense, meshes in nicely with some recent work in psychology, and has the merit of being, in principle, fairly easy to test.

It appears that shape is a dimension of some importance for children in acquiring the meanings of fairly simple words. Eve Clark (1973), in a survey of some traditional sources on the phenomenon of 'overextension' of words by children - the generalising of a particular feature of the meaning of a word so as to cover objects not normally within its extension - reports that she "found that the majority of the features first used by children in overextension seemed to be directly derived from their perception of the object named" (p. 29). She claims that "this reliance on a limited set of perceptual attributes for categorisation. . . . seems to be operative primarily during the first year of learning to use language" (p. 30). Interestingly, the examples which she quotes are those concerned with roundness: the word used by

the child for 'moon' becomes generalised to cakes, round marks on windows and in books, round postmarks and the letter O. Another example, from Georgian, concerns the word for 'ball' which becomes generalised to toy, radish, and finally the stone spheres at the entrance to a park entrance, this perhaps being not too unexpected. Clark also quotes work showing that children 'used shape as their main criterion in free sorting tasks", and that "form (shape) was preferred in paired comparisons over both colour and size" (p. 33).

More substantive are the interesting experiments reported in Rosch (1973)a. The Dani are a stone-age New Guinea tribe whose language apparently possesses no individual items designating shape or colour categories, and in pilot tests they were found to be not very good at communicating these concepts to each other verbally. On the basis of earlier work, Rosch hypothesised that there are "natural prototypes" in the domains of form and colour, that is, best examples of, say, red or green, or circle or square. So, for example, an egg-shape will be judged a better example of the category 'circle' than an irregularly drawn circle with a large gap in it. The same thing applies to other, non-perceptual domains: see Rosch (1973)b and Lakoff (1972) for some discussion of the linguistic import of these findings.

Rosch set various learning tasks for Dani subjects, in order to test the major hypothesis that "there are non-arbitrary color and form categories which form around perceptually salient "natural prototypes" ". She came to the following conclusions: "(a) Subjects from a culture which did not initially possess hue or form concepts could learn names for the presumed natural prototypes faster than for other stimuli even when the natural prototypes were not the central members of the categories (b) members of that culture could more easily learn hue and form concepts when the presumed natural prototypes were the central members of categories than when the categories were organised in other ways, and (c) at least for the forms, subjects tended to 'distort' their definition of the category towards the natural prototype; that is, they tended to choose the natural prototype as the most typical member of the category even when it was actually peripheral" (1973:348). As regards the domain of colour Rosch claims that this demonstrates, in part, "the way in which perceptual-cognitive factors (the salience and memorability of certain areas of the colour space) can influence the formation of linguistic categories" (348). And she adds: "In the present study, form categories were shown to be influenced by natural prototypes in much the same way as color categories" (349). It seems to me that the "psychological salience" hypothesis has considerably greater explanatory potential than its rival, if it can be given some measure of confirmation. It is at least fairly clear how we would set about testing it: the hypothesis would predict that speakers of

a language with a classifier like 'long' or 'round' would agree to a significant extent on "best examples" judgements. These one would expect to coincide with the 'natural' or 'good' forms discussed by Rosch herself, for example, a regular circle. For 'long' we might expect something of about cigarette dimensions, perhaps, though this would have to be ascertained experimentally. We would further predict that in a test situation in which speakers were confronted with new or artificial objects and were forced to assign a classifier to them (in the course of describing them), reaction times would be significantly faster for those objects which more closely approached the 'natural prototype' of that category than for those which were peripheral to it, even where there was no question of a decision between different categories or classifiers. We might also expect a higher measure of agreement among subjects as to the classifier chosen for such instances, in case of differences like this arising.

What I hope to have shown, then, is that there are some universal features of shape in classifying languages, and that a psychological explanation for these in terms of "natural categories" appears to be a plausible approach.

I am grateful to David Kilby and Martin Atkinson for some useful discussion and references.

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Since all possible combinations of ±long and ±rigid occur, as well as minimal discriminations requiring each feature alone, there is no principled reason (when this subsystem is considered in isolation) for maintaining that one is in any way prior to the other. Our amended weak hypothesis is still consistent with this data, fortunately: it claims that the feature of rigidity and whatever dimension it is part of, only appears when the dimension of shape appears. Thus it claims that the dimension of shape, and the particular categories within it, are more salient, on the whole, than the dimension of which rigidity is a category. This hypothesis, then, would be refuted by the existence of a language which made use of the ±rigid distinction without making use of ±long or ±round, etc.

Acoma is a Keres isolate of the Hokan Siouan phylum. It has what appear to be a restricted set of classifiers. (Miller 1965) "The classificatory verb stems constitute a small group of stems that undergo special thematic derivations and take special non-thematic suffixes. Some of the stems are defective and many of the formations are irregular" (132-3). They are all concerned with the notion of handling: the classificatory element determines what it is that is being handled:

1) to handle things in a basket
2) to handle liquid
3) to handle things in a sack or box

} most regular and frequent

4) to handle grainlike things (or sandlike)
5) to handle meat
6) to carry on the back
7) to handle one flexible object
8) to handle several flexible objects
9) to handle one compact object
10) to handle several compact objects

Despite the paucity of data, it seems clear that insofar as this is a system, it contains the features ± enclosed, grainlike/liquid/solid (?), sing/plur, flexible and compact. Is flexible opposed to compact? It isn't possible to say with certainty, but it looks very much like it. In any case, the existence of flexible and the absence of ± long serve to disconfirm even the weakest of our earlier hypotheses. Of course, we could always argue that this isn't a 'true' system, and it is certainly not as well developed as most I have looked at, but in the absence of any clear criteria as to what infallibly constitutes a 'true' system, this sounds more like explaining away. Rather, I think we must conclude that the hypothesised priority of ± long over ± rigid is only a tendency, and not a strict universal, whether or not they are separate dimensions.

Diegueño, a Yuman language of the same phylum as Acoma, also has a system of classificatory verbs of sorts. Verb stems in this language

Table 1.

	Abl.	Pros.	Instr.	Mat.	Comp.	Reas.	Part.	Comit.	Trav.	Topic.
Amharic [1]	ka		ba	ka	ka	sil/ba	ka	kanna	ba	sil
Azerbaijani	dan	ilə	ilə	dan	dan	dan	dan	ilə	ilə	dan
Carib [2]	*wiino*	*po:ko*	*wa*			po:ko	me	po:ko		po:ko
English	from	by	with	with/of	than	from	of	with	by	of
Eskimo	miŋ	kun	miŋ	miŋ	miŋ	miŋ	miŋ		kun	erg.
Evenki	duk	duli	da	duk	duk	duk/da	duk	da	da	duli
French	de	par	avec	de	que	de	de	avec	par	de
Hungarian	bol/ rol/tol	visz	vel	bol	nel	tol	bol	vel	vel	rol
Russian	iz	instr.	instr.	iz	gen	iz	iz	s	instr.	o
Twi [3]	firi	fa	de/fa	de	sen	nti	bi	de	de/fa	fa
Xinalug	li	li	li	li	li	li	li	li	li	li
Erz'a [4]	de	ka	so	de	de	de	de	marto	so	de

1. Data from Cohen 1970
2. Data from Hoff 1968
3. Data from Christaller 1875
4. Data from Koljadenkov 1959

of languages of being in the ablative rather than the prosecutive group (in Kabardian, there are two case forms expressing the instrumental, one identical to the ablative, the other identical to the comitative).

What follows from the data I have considered? It is immediately clear that a purely surface syntactic approach to cases can have nothing to say about these phenomena; since it is not clear to me that there is anyone who would still claim that (although it is the position of Chomsky in 1965 [p. 221]), this conclusion is not likely to disturb many linguists. The number of promising alternative proposals is not, however, very great. The most obvious candidate, as we have seen, is Case Grammar in one or other of its varieties. But I know of no proposal which would account for even a small part of the data presented here.[14] A number of possibilities could be considered by the case grammarian attempting to deal with this problem. He could postulate two cases (deep cases) to cover the two groups of relations isolated here, and establish conditions of realisation of these cases in particular languages. This would be unfortunate in three directions; firstly, it would be circular, establishing cases on the basis of the morphological identity for which it is trying to account: secondly it would be semantically unrevealing, in that these new cases would not easily be interpretable as having any semantic unity - and semantic unity is one of the advantages which deep cases are supposed to have over other syntactic devices: thirdly, there would still be many overlapping loose ends - some case forms would move outside the boundaries of the deep case which primarily covers their use, and other case forms would be purely representative of one sub-use of the deep case as a whole.

Another possibility adumbrated by Starosta is that deep cases be represented as feature matrices, and the identity of various case forms be accounted for on the basis of identity of feature rather than necessarily on the identity of case. Apart from the general undesirability of having recourse to features with their excessive flexibility, this suggestion would seem to suffer from the same sort of circularity as did the idea of creating 'super-cases', and it is not easy to envisage a feature representation dealing with this area which would not be intolerably ad hoc and unrevealing. Any such approach seems to show up the tension which characterises much Case Grammar work between semantic motivation and syntactic relevance; an increase in one often entails a corresponding decrease in the other, while if the two are not compatible, case grammar ceases to be an interesting hypothesis.

[14] The only proposal which could be interpreted as relevant is Anderson's suggestion that the Ablative and Ergative cases be merged. (1971 p. 175).

This in my view is the situation if the case grammarian takes a third point of view - that adopted by Starosta and criticised in §3. Universal correlations which are at the same time ad hoc seem to constitute some sort of contradiction. Their total lack of explanatory potential seems clear; what is not clear is whether they are even empirically adequate. If it were a question of form-meaning correlations which are absolutely universal (there may in fact exist such), or even of some sort of hierarchical relationship between case forms and case uses, then Starosta's mechanism would at least be able to cope with the facts. But the facts seem to be somewhat messier; correlations of the sort: if the case uses x and y are identical in form, then the case uses x and z will also be identical in form, do not appear to be possible. What is needed is some sort of approach which will predict the likely coincidences of form while allowing for all the actual coincidences of form with a greater or lesser degree of probability. This is probably asking too much, but must be the goal which is aimed at. I find it difficult to imagine that such an approach could be other than a semantic one (or at least largely semantic), and I shall try to look at some of the possibilities which seem to me to be more or less plausible.

7.1 It is worth setting out some of the informal semantic similarities among some of the case uses in this group in order to see rather more clearly the possibly contradictory ways in which this problem may be approached. At the same time an effort must be made to avoid imposing the categories of a particular language on the problem, for instance by using the term 'source' in English as if it explained the affinity between the ablative and the causer of a causative expression: of course the meaning(s) of the word 'source' is (are) simply a further reflexion of the same phenomenon as conditions the identity of the case forms. We have already established that this cannot be arbitrary, so this further evidence of that is of minor importance.

Anderson (1971) notes the affinity of the ablative with the negative in a morphological sense - cf. the use of from in English with verbs whose complement is semantically negative, such as prevent. This is not simply a morphological fact, but correlates with the semantic fact about the ablative that it has implications of a negative kind - i.e. if one comes from somewhere, then one is no longer in that place. It is possible that this semantic fact could be structurally incorporated in the semantic representation of the ablative. The incorporation of a negative has also been proposed for the semantic representation of comparative structures, e.g. by Seuren (1973), again, on grounds that are both syntactic and semantic. The syntactic grounds include the fact that many languages (including, for example, French) regularly have a negative element in the clause of compared state, and others (e.g. English) have negative diagnostic features there (e.g. anyone in John

is cleverer than anyone else I know). The semantic grounds are based on an analysis of comparative sentences which associates them with a structure such as:

Np is Adj to an extent to which NP is not adj.

A further line of thought which can to some extent relate these analyses to the partitive involves the following factors: the partitive is essentially a relation between elements, one of which must be a quantifier-like element of some sort. Negation is a limiting case of quantification, and therefore all of these case uses are inherently linked to some extent with quantifiers of one sort or another. This perhaps does not lead us to understand in any more detail the precise mechanisms by which quantification and case assignment might be related, but it does allow us to generalise over a number of these relations, which is still quite useful. What it does not do is help with the analysis or identification of the reason or material case uses.

A further fairly straightforward parallelism which might well have some explanatory value is the association of agentive, instrumental and reason expressions as special cases of causation. 'Reason', in the sense I have used it, is wholly subsumed under the notion of causation, and instrumental and agentive, although not wholly coinciding with this notion, are nonetheless very closely connected with it (a verb such as read is an obvious exception to any total identification of the two concepts). Any causative verb will generally be capable of combining with an instrument, and if there is any special representation of the agent, this will inevitably apply to causative verbs.

The close relationship of the instrumental, the comitative and the prosecutive may also have an explanation in some sort of abstract semantic features which they all share: for instance, the prosecutive is often analysed as the combination of allativity and ablativity in the one term. In a similar sort of way, the instrumental may be looked upon as a relation in which the term acts upon another, at the same time as being acted upon itself by another. To exemplify, the idea of 'going through' a place combines the ideas of 'going to' and 'leaving' that place, while the idea of 'doing something with something' [instrumental] involves the agent doing something to the instrument, and the instrument doing something to the patient. This sort of abstract relationship would clearly not be best captured by any sort of traditional semantic 'feature', but can naturally be looked on as an abstraction from two sets of different semantic relations. The place of the comitative in this representation is not so clear, but it is possible that it too may be analysed in rather a similar way: the comitative term is involved in doing something but at the same time is associated in some sort of locative way with the other agent of the predicate.

The association of the expression of reason with that of the ablative could be considered from the following point of view. If one takes the fairly common notion of causation as involving a relation between one predication and another (as opposed to it being a relation between a predication and a thing [the causer]), then it can be represented as involving an 'antecedent' and a 'consequent' situation linked by a causative predicate. Thus, John died from typhoid could be represented:

[John have typhoid] CAUSE [John die].

A similar state of affairs may be said to hold for ablative sentences which are completely explicit, as in: John went from London to Manchester. This could possibly be represented as:

[John be in London] BECOME [John be in Manchester].

Here again, although the specific relations that are employed differ in these two sentences, the similarity of structure is evident if one abstracts from the specific features and talks simply in terms of 'antecedent' and 'consequent'.

The connexions drawn in this section have been little more than suggestive parallels suggested by morphological parallelisms noted in earlier sections. It would no doubt be possible to concoct such parallelisms also for relations which never display such morphological similarity, in a sort of reductio ad absurdum of this argument. This, however, would be to miss the point; the relationships suggested here are intended to be the starting point of an attempted explanation of these phenomena, rather than in any way to constitute an explanation in their own right. The data presented above will not, it would seem, support a single universal analysis of the case system of language in general. What it does show the need for is a system of non-arbitrary characterisations of the case systems of individual languages insofar as they conform to the pattern shown here. The motivation for this is as follows: I claim that the regularities demonstrated here are sufficient proof of the non-arbitrariness of the particular realisations of cases in language. For instance, the morphological identity of the ablative and partitive in Azerbaijani cannot be arbitrary because the same identity is found in other genetically and geographically unrelated languages. This is not to deny that there could possibly be arbitrary associations of cases in a single morphological form, nor to deny that there could also be cultural reasons for the identity in a given language of two case relations. The point is simply that the data brought forward here (especially if it is supported by more evidence) demonstrates that there are certain case relations which combine in a single morphological form in a way that can be neither arbitrary nor culture-bound.

7.2 It must also be noted that in the form in which it has been presented, the set of suggestions I have made do not constitute an empirical hypothesis, because they do not explicitly exclude any relations from occurring together. It is quite clearly out of the question to suggest that all languages must have any of these combined relations, because even the small body of languages I have looked at fairly superficially does not confirm this. As it stands, however, there is no combination of case relations which would be counted as counterevidence to my proposals. I have suggested that the set of ten case relations considered here in some sense forms a natural class, while another natural class could include such case relations as allative, dative, purpose, benefactive, etc. Only when there is some statement excluding combinations of case relations as well as others suggesting the possibility of such combinations can it begin to be an empirically verifiable enterprise.

For obvious reasons, on the other hand, it is less easy to collect examples of non-coincidence in unfamiliar languages. A perusal of grammars will always yield examples where the form of two different constructions is identical. But even with native speaker informants it is not always possible to exclude certain sorts of coincidence as possible, because it is not always straightforward to define the precise limits of any particular construction. To take a trivial example, it is usually possible in English (and also apparently in many other languages) to express the meaning I have been referring to as 'means of transport' by a case form from the group I have been describing, or alternatively by a case form from the straightforward locative group (thus 'go by plane', 'go in a plane'). If one were to claim that the ablative/prosecutive group and the locative group were in general mutually exclusive, then such examples might at first seem to constitute a counterexample; yet they are not very satisfactory counterexamples - the relationship of means and the relation of location are both valid in this sort of situation, yet they can not really be taken as meaning the same thing (in some suitably vague sense). Yet one can well imagine a situation where only one or other could be elicited by the linguist from his native informant: such problems obscure the status of counterexamples.

What I believe to be demonstrated by the data I have presented is quite simply that patterns in the expression of case relationships cannot be characterised as arbitrary features of individual languages. It is possible to make the strong claim that a universal account of case relations can be written - a sort of grammar with gaps left for the variable features such as the specific realisations of cases. On the basis of the data I have presented I cannot justify more than a somewhat weaker hypothesis - that the widespread nature of a particular case syncretism invalidates an approach to a particular language under which that particular syncretism is treated as arbitrary. This position seems

to me entirely reasonable - its weakness residing in the fact that it allows for arbitrary or language specific (possibly culturally determined) syncretisms as well (those that are not supported by the case systems of other languages[15]), and also that it does not force us to maintain an identical account of each individual syncretism. A hypothetical example of this might be a syncretism of two case uses in a single form of one language because of a common semantic feature, while in another language it might be a subpart of a more general syncretism motivated by an altogether more general feature.

It is therefore a test of a universal approach to cases that it should assist in the construction of grammars for individual languages. I shall therefore attempt to offer two sketches of how case patterns may be accounted for, not as explicit grammars of parts of languages, but more as possible suggestive comments towards a more comprehensive account. It should be stressed that these sketches do not form (even notionally at present) part of a more general outline of a grammar, and that they are therefore to be taken more as indications of what might be done in an attempt to describe the cases of a language in a unified manner, rather than a coherent attempt in their own right.

8.1 Russian expresses the case uses described in §5 largely by the ablative[16] and the instrumental: other cases and prepositions overlap here as well, some covering more than one of the case uses - further confirmation that some of these are related. The instrumental case without preposition has more separate case uses than any other, a fact which has led many people to propose that it cannot be described as a single case, e.g.

> "We have become used to talking about a single instrumental case in Russian, but in fact that case is not a single grammatical category, but several different genetically related categories. Every use of the instrumental is a new case, so that we have several cases called instrumental". (Potebnja 1888 p.443, cited approvingly by many Soviet linguists).

This rather negative attitude is widely held in the practise of the

[15] Of course, this still leaves two problems (at least); how many examples we need to provide the requisite support, and how we can ever exclude the possibility that some motivation may be found for any syncretism.

[16] In Table 1 I have mentioned only the preposition iz, but the term 'ablative' includes also the prepositions ot and s, all of them with the genitive. Comitative s governs the instrumental case.

syntactic claims made on the basis of case grammar. The first point can be illustrated with some claims that Starosta makes about English; one such claim is that by in English can express Instrumental, as well as Agentive and Locative. One must assume that he has in mind here such uses as "Bill was hit by a piece of flying glass' or 'The door was opened by the wind'. Considerable argument has gone into this point (cf. Fillmore 1971), but it is nonetheless the case that the analysis of such sentences as containing an instrumental is heavily dependent on the adoption of a certain theoretical position which is itself highly arguable. An even more striking example of such dependence is Starosta's claim that the dative, directional and locative cases in English can all be realised by to: the only example he gives to support his contention that to can be a locative is the sentence 'The book is stuck to the chair' (p. 2). As there is no specific argumentation on this point, it is difficult to understand the motivation behind this case-assignment. In a sense, this is simply quibbling about details, but if such doubts can be thrown upon Starosta's (or any case grammarian's) analysis of the case forms of his own language, then there must be considerable doubts as to the analysis of the case forms of less familiar languages. It is my contention that deep cases are too abstract and unspecific a concept to be of much use in a cross-language comparison of this sort, and that the comparison of case uses (in the sense of § 1) is greatly superior methodologically.

A number of other doubts which arise in Starosta's article make it very difficult to accept even that it is anywhere near the right lines in trying to solve the problem that it poses. He proposes a number of 'universals' which are either vague due to the indeterminacy of the case notions used (e.g. all languages can have dative and objective direct objects), or vitiated by quirks of analysis, as shown above, or dubious generalisations: for example, he claims that all languages that he knows of have a fairly easily identifiable grammatical subject (p. 9), and later that the Objective case is the subject in an ergative language (p. 13). But there is considerable (if not absolutely conclusive) evidence that in most ergative languages the notion of subject, if it is to be taken as having any syntactic (as opposed to morphological) relevance, must be applied to the constituent in the ergative case (cf. Comrie 1973). Further, his claim that it is necessary to assign labels to case forms, and that these just happen to turn out to be similar to traditional labels for case forms, is deprived of any empirical content by the way in which he chooses to define the labelling of a case form. This is done by taking a particular syntactic or semantic relation and stating that the case that expresses this relation (whatever else it also expresses) is given a certain label (e.g. nominative, locative). Insofar as all languages will express a certain meaning, then it is clear that there is no possibility of them not

having the corresponding case form according to this definition.[5] The claim is therefore empty. It is sufficient, in my view, to talk about identity of forms without having to label them: if the meanings generally associated with the words locative and ablative are realised in a certain language by a single case form, then it is purely academic to argue whether or not this form may be called a locative, an ablative, or something else. The relevant information is simply that there is a form such that both of these meanings may be expressed in that form.

4. What Starosta's article does, however, show quite clearly is that there is a substantial and central area of grammatical structure which, in spite of extensive investigation in traditional and (to a lesser extent) transformational works, defies any coherent theoretical interpretation. I have explained why Starosta's proposal is little better than its predecessors from a theoretical viewpoint, but it has performed an undoubted service in forefronting this problem. Two alternative approaches may be rejected immediately: one would be that these coincidences in many languages are simply a reflexion of remote historical relationships between languages. These facts would then not be of relevance for linguistic theory, but be purely arbitrary synchronically; such a theory must be rejected because the historical relationships posited would have to be so remote as to be untestable. Even if it corresponded to fact, there would be no way of discovering this, so a more explanatory solution is called for.[6] A variant of this theory, which is even less desirable, is that certain case relationships reflect innate (but arbitrary) connexions of language representation: I take it that such a theory is too absurd even to be discussed. This first possible approach is something of a straw man, although I cannot imagine how a standard transformational grammar could approach the data I am referring to in any other way; but there is another approach which is adopted by a number of linguists (including Starosta to some degree), which claims that there is a semantic basis for these formal coincidences. If the Instrumental and the Comitative are similarly

5 Of course, if the meanings of two 'traditional cases' are expressed in one case form in some language, then not all of these cases can be claimed to exist in that language. But Starosta is not claiming that all languages have all of these superficial cases. There might also be the possibility of a language dividing case relations up in such a fashion that each relation would be expressed by many case forms: Starosta's theory would exclude that if anything - but even that is not certain.

6 This is not to say that no case syncretisms are based on historical relationships: Starosta provides some very tentative evidence that some are.

expressed in a large number of languages, then it must, according to this theory, follow from the fact that they have some semantic feature in common (cf. Nilsen 1973). The Ablative and Agent coincide formally with some frequency, so they may be united in a single deep case relationship (Anderson 1971). There is clearly a place for such suggestions, as the boundaries of a single deep case relationship are extremely fuzzy, but there is a danger in too wide a postulation of such identities - whether they are called identities of deep cases or of semantic features of some sort. The most obvious danger is that they lead in many cases to particularly unrevealing semantic bases; for example, it is not wildly counterintuitive to postulate some common element of 'association' for the instrumental and the comitative, but, on the other hand, it is not wildly counterintuitive to claim that any case relationship involves 'association'. It is perhaps not unreasonable to demand that any semantic features which are used in this way should be independently motivated, preferably with syntactic consequences, and that case uses identified in some sense should be identifiable by use of such independent procedures.

Examination of case forms in a number of languages will virtually always emerge with examples of case forms which combine a wide variety of meanings: many of these examples will include meanings which could hardly be said to be the same in any sense at all, although such is the power of linguists' imagination that there will always be someone who is prepared to find a common semantic feature in the most unlikely sets.[7] All this is not to deny, however, that there may be a single defining feature for the whole set of uses of a single case form in a language; I am simply claiming that such a feature cannot in many instances be a simple-mindedly semantic one. Nor is this a statement of an anti-semantic bias, as it is also clear that a simple-mindedly syntactic approach would fare even worse in an analysis of case forms - even with such 'syntactic' cases as nominative and accusative (in the sense in which these terms are used with respect to Indo-European languages), there must be some way in which their semantic range may be specified. I shall therefore, on the basis of a set of uses formally related to the ablative[8] in a number of languages, present an analysis which will suggest, in spite of its manifest incompleteness even within the area covered, that there is an indirect semantic motivation for the

7 Martinet (1975) claims to detect in the meanings of the French word fraise ('strawberry', 'ruff', 'dentist's drill') the common semantic feature [unevenness].

8 By 'ablative' I mean a case use expressing the idea of physical motion away from some object. I here abstract from such features as spatial parameter ('top', 'inside', 'front', etc.).

realisation of case uses in certain case forms.

5.1 The ablative in Azerbaijani is expressed by the case-suffix -dan (which may be altered slightly by such means as vowel harmony).[9]

1. Biz bağ -dan endik. 'We hill-from came-down'.
In addition to this, its (apparently) basic meaning, the suffix -dan is also used in expressions of reason, comparison, the partitive, material (= 'made of'), topic of speech and apparently also prosecutive: e.g.

2. isti-dən tərləmişdim. 'Heat-from I-sweated'.
3. Baky Kirovabad-dan böjükdür. 'Baku Kirovabad-than is-bigger'.
4. tələbə-lər-dən biri. 'Student-s-from one' ("one of the students").
5. evi ağač-dan tikdilər. 'House wood-from they-built'.
6. səfir neft-dən danyšdy. 'Envoy oil-from (i.e. 'about') talked'.

The primary expression of the prosecutive (motion through, past, over, etc.) is the postposition il, which can also be used in expressions of instrument, comitative and means of travel, e.g. :

7. Külšən küčə ilə kedərək. 'Gjulshen road along went'.
8. əli ilə išarə... 'Hand with pointing ...'.
9. siz ilə bir jerdə jašajyr. 'You with one place-in she-is-living' (i.e. 'she is living together with you').
10. təjar ilə uč-mag. 'Aeroplane by travel-inf'.

We see, therefore, that Azerbaijani expresses the ten case uses listed above with the aid of two, partially overlapping case forms. In itself, this fact is not overwhelmingly significant; it appears, however, that Azerbaijani is far from isolated in expressing approximately these ten case uses with a small number of forms. A large number of languages with no known genetic relationship manifest a considerable degree of overlap in precisely these case uses. There are a number of other case uses which appear to fit in the same pattern, for instance, the expression of the agent in passive sentences, and the non-direct object of verbs in the same semantic class as seize or grasp (e.g. seize someone by the hand).

5.2 Xinalug, a North-East Caucasian language, has a number of separate cases (case suffixes) expressing various subtypes of ablative as their primary meaning. But all ablatives have the common ending -li, while the rest of the case ending which precedes this suffix expresses the corresponding locative case use. One case ending in -li has no locative counterpart - this is termed by Kibrik et al. (1972, all data here is from this source) the comitative case. I am here only concerned with the fact that a form ends in -li, so this lacuna is not significant. As in Azerbaijani, the relations involved are the ablative,

[9] Data from 'Grammatika' 1971.

the prosecutive, instrumental, material, comparative, reason, partitive, comitative, means of travel and topic of speech:

11. tsuaxili lixi. 'From-the-house go-away'.
12. zω sæ tsuaxili lakuet^{h}mæ. 'I that house-past go'.
13. p^{h}ωlωli p^{h}ʂi tuvunkuidæmæ. 'With-money horse one-can-buy'.
14. dæ tsua k'atʂ'ωnωʂili mæ. "That house of-stone is".
15. vats'ωq'ili ys vixæ ʒmæ. 'Than month year longer is'.
16. zω sasωʂili ailmωʂq'iʂæmæ. 'I from-noise woke-up'.
17. xωnωʂili sa t'ωng t^{h}æ. 'Of-water one drop give (me)'.
18. zω p^{h}ʂiʂkili kuet^{h}mæ. 'I with-horse am-going'.
19. zω k'alali p^{h}ʂili k^{h}abudmæ. 'I from-Kuba by-horse came'.
20. dædi ħæjæliʂili tʂ^{h}ωk'ʂæmæ. 'Mother about-child talked'.

With all of these case uses, we can therefore see that there is at least one way to express them using the ablative ending. This fact again is not of great significance in itself, but along with the Azerbaijani data it is enough to give rise to some suspicions that these case uses are not simply an arbitrary list.

5.3 Eskimo has an ablative ending -miŋ and a prosecutive ending -kun[10]; between them they express eight of these ten case uses I have isolated, with the other two being expressed otherwise[11]:

21. xwaŋa sna-miŋ tagiquŋa. 'I shore-from came'.
22. mikilɣihaq igaɣaquq igasi-miŋ. 'Boy is-writing pencil-with'.
23. apaka unɣanljaɣaquq unahsi-miŋ. 'Girl makes-harpoon wood-from'.
24. kajna iknaquq ama-miŋ. 'Brown-bear is-stronger wolf-than'.
25. nipsug-miŋ iɣumakut gujguŋwaɣmun. 'Rain-from we-hid in-shed'.
26. maku-niŋ kartina-niŋ tugugu atasiq. "These-of pictures-of take one'.
27. aŋa-kun naluhqutaɣaquq mitihljuk. 'Kayak-along hops bird'.
28. aŋligutka kasimaq tiŋixkajux-kun. 'My-brother arrived plane-by'.
29. uŋipamsukaqixkaŋa Aliŋtituŋam. 'He-talked Alintitun-about'.

[10] I am here talking about Asiatic Eskimo - data from Menovščikov 1962/7.

[11] The comitative is not expressed by a case ending at all, but topic of speech is expressed by the ergative case, which also serves for the expression of the agent in a transitive sentence, and also phrases of reason:

a. jugim aglataqa qikmiq. 'Man-erg. walks dog-abs'.
b. qanigim utkusiput anigum asiŋanun ligumat. 'Snowfall-erg our-traps snow under remained'.

5.4 Evenki (Tungus) manages to express these ten case uses in three case forms, the ablative (-duk), the prosecutive (-duli) and the instrumental (-da):[12]

30. bi djŭ-duk-pi jŭm. 'I house-from-my left'.
31. poest ŏmaktalĭ tigdələ̄n-dulĭ ŋənəran. 'Rifle-with dog I-hit'.
32. pəktirə̄vun-də ulukĭvə iktəm. 'Rifle-with dog I-hit'.
33. amākāv mŏ-duk abdukānəv ōran. 'Grandfather wood-from toy made'.
34. xəpətə ullən mŏtĭ ullə-duk-in ajatmar. 'Bear its-meat deer meat-than-its better'. (i.e. 'bear meat is better than deer meat').
35. xutəlvun ajaduk debə-duk abgarāl baldĭdjapkil. 'Our-children good feeding-from healthy grow'.
36. pionerĭl-duk-pun ajat učitčān Tamara. 'Pioneers-from-our well studies Tamara'.
37. alagŭvdjarĭl učitel-di-vər əkskursijalā sururə. 'Pupils teacher-instr-their on-excursion went'. (i.e. 'with their teacher').
38. bu djavil-di-tin əĕkĭ surusinəv. 'We boat-instr-their downstream went'.
39. učitel kuŋakārvə Lenin in-dulĭ-n ulgučənən. 'Teacher children Lenin life-prol-his told'. (i.e. 'about his life').

5.5 French uses three case forms (prepositions) to express nine of these case uses, with only the comparative expressed by a unique form:

40. Il est venu de Paris. 'He came from Paris'.
41. Il est passé par Paris. 'He passed through Paris'.
42. Il l'a ouvert avec une clef. 'He opened it with a key'.
43. Il est arrivé par bateau. 'He arrived by boat'.
44. Il est allé se promener avec son chien. 'He went for a walk with his dog'.
45. Il est mort de faim. 'He died from hunger'.
46. Chacun d'eux sait nager. 'Each of them know how toswim'.
47. Il a parlé de vous souvent. 'He often spoke of you'.
48. Il a fait un table de bois. 'He made a table of wood'.
49. Jean est plus grand que Paul. 'John is bigger than Paul'.

French also has constructions where de and par alternate in a fairly complex way, as in the agent phrase of a passive construction. This sort of interweaving of case forms is what one might expect given the homogeneity of the area of case expressions being considered here, although it is clearly too complex for a superficial treatment such as the present one[13].

[12] Data from Kolesnikova (1966).

[13] It might also be significant that the ergative case in Eskimo (cf. fn. 11 above) and the preposition de in French are also used to express possession in attributive position:
a. ajviğim nasqwa. 'Walrus-erg head-its' (the walrus's head).
b. la plume de ma tante. 'The pen of my aunt'.

5.6 The data considered in the previous five sections seems to provide rather strong evidence that the choice of these ten case uses (with the possibility of a few more) has to be very far from arbitrary, even though they do not immediately seem to have a great deal in common. More evidence of a similar type could readily be provided, and Table 1 summarises the data given, as well as adding data from a number of other languages in purely summary form. It is clear that not all languages combine these case uses in such a small number of forms as those I have considered up to now. I shall not go into this question in detail, but it would seem from the data I have collected so far that this is simply a function of the number of case forms available to express the number of case uses; languages with a few cases and few post-positions or prepositions will obviously include more case uses in one case form than languages with large numbers of prepositions or post-positions or cases or serial verbs, etc. Many other problems also arise with respect to the heterogeneity of the case uses which I have defined, and the multiplicity of expressions for these which most languages appear to have. But I shall not consider these difficulties in any detail, but continue to look at the data I have given as if it reflected the true situation of the expression of these case uses in these languages. I shall examine in rather more detail later the implications of the analysis for the description of the case systems of individual languages.

A cautionary note must also be added; I have not attempted to ensure that every means of expressing these particular relations is noted here, nor that all relations expressed by these forms are recorded. These paragraphs are intended as suggestive examples rather than as detailed analyses of the case structure of particular languages. Slightly more detailed sketches of two languages are found in §8 below, in which at least some attempt is made to incorporate both the range of meaning of individual case forms, and the variety of case forms expressing a particular case use.

6. A number of patterns emerge with some clarity when the data presented in Table 1 is examined. First of all, there is a marked division into two groups, one which definitely contains ablative, reason, material, partitive and comparative, the other containing prosecutive, instrument, comitative and means of travel. The expression of topic of speech leans towards the ablative group, although this is not as clearly marked as the other case uses with respect to its grouping. The prosecutive does not coincide in its case form with reason, material or partitive except in Xinalug, where it also coincides with the ablative. None of the languages examined here contradict this grouping in any serious way, so it must be presumed to have some special status with respect to the interpretation of this data. The only qualification that seems necessary here is that the instrumental shows signs in a number

Theoretically Significant Word Order Patterns in Greenberg's (1963) Appendix II

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Introduction and Abstract

One of the most important contributions which has yet been made to the study of universal grammar is Joseph Greenberg's (1963) paper "Some Universals of Grammar with Particular Reference to the Order of Meaningful Elements". Using a 30-language sample with a wide genetic and areal coverage Greenberg sets up 45 universal statements of a syntactic and morphological nature. The great majority of these are implicational statements of the form, if a language has some property P then it also has some other property Q. Some of the 45 universals are presented as "statistical" statements, admitting of a limited number of exceptions, though most are given as "non-statistical" or exceptionless. The syntactic universals all relate to word order. Greenberg proposes three universal word order types, Verb-Subject-Object (VSO), Subject-Verb-Object (SVO), and Subject-Object-Verb (SOV), and argues that these different positions of the verb relative to subject and object correlate in a principled way with word order patterns elsewhere in the grammar. In a separate appendix, Appendix II, Greenberg then gives some basic word order information on a considerably expanded list of the world's languages.

Greenberg (1963) can be seen as a landmark paper in two respects. It established the validity and importance of a whole new type of universal statement, the implicational universal, and thereby set a precedent for the discovery of other universals of this logical form, e.g. the Accessibility Hierarchy of Keenan and Comrie (1972). And it also provided the groundwork for much subsequent work on word order itself. This subsequent work has gone beyond Greenberg (1963) in roughly three ways. There has been, firstly, a reformulation of many of the universals; secondly attempts have been made to actually explain the word order patterns which are presented simply as observational principles by Greenberg; and thirdly, Greenberg's typology has become widely used in a historical context, both as an aid to the reconstruction of proto-languages, and in historical theories explaining why word orders change. The work of Theo Vennemann (cf references) exemplifies all three developments and has attracted particular attention.

In this paper I am going to focus attention on Greenberg's (1963) Appendix II. This appendix gives information on four basic word order properties: the position of the verb relative to subject and object (VSO, SVO or SOV); the adposition order (i.e. preposition versus postposition); the order of the genitive relative to the noun (NG versus GN); and the order of the adjective relative to the noun (NA versus AN). There are 24 logically possible co-occurrences of these different properties, and Greenberg assigns each of the languages he has studied to the appropriate co-occurrence type. The number of languages represented is extremely

large. 122 languages are named individually, and in addition whole families and subfamilies of languages are referred to. The following linguistic groups as a whole are assigned to the appropriate co-occurrence types of their members: Celtic, Romance, Polynesian, West Atlantic, Bantu Thai, Finno-Ugric; and information is given which is claimed to be valid for most Benue-Congo, Chadic, Mandingo, Voltaic, and Australian languages, and for many Austronesian, Ote-Mangue, Daghestan, Caucasian and Sino-Tibetan languages. As a result, Appendix II contains basic word order information on the biggest and most representative language sample yet collected. But despite this vast coverage, Appendix II has not yet received the attention which I believe it deserves. The number of properties covered is, admittedly, small compared with the extensive properties for which the 30-language sample was tested. And in addition, the overall impression which Appendix II first makes upon the eye is slightly messy. Patterns are not exactly jumping out of the data.

And yet I am going to argue that Appendix II, despite this first impression, does afford insight into universal principles of word order which are theoretically very significant at this time. It can provide crucial information of relevance to current disagreements and unclarities in word order studies. And it suggests some new principles of considerable potential generality in describing, and ultimately explaining, cross-language word order patterns.

The current state of word order studies is actually very unsatisfactory. There is widespread confusion over the nature of an implicational statement as such. There is disagreement over the statistical or non-statistical status of these universals. There are competing universal formulations describing basically the same body of generally available data, some of which show a complete disregard for vast numbers of exceptions. There is disagreement over the number of basic and hence significant language types which are claimed to exist universally. And many historical theories explaining why word orders change are not only quite at variance with synchronic evidence, they can be shown upon examination to be theoretically self-contradictory and empirically diametrically opposed to such historical evidence as is available (cf Hawkins (1976)). Add to this the intrinsic difficulty that exists in classifying many languages as VSO or SVO etc. in the first place, and one can only despair at the prospect of achieving any significant contribution to general linguistic theory in this area.

And yet anyone who has looked extensively at word order facts synchronically and diachronically cannot escape the powerful impression that there are indeed regularities at work linking cross-language word order patterns to one another, even though current statements are

inadequate in capturing them. So how can we decide between current disagreements, and where do we go from here? I suggest that we return to the data and look closely at the biggest language sample we have, Greenberg's Appendix II. Any regularities and insights which can be gleaned from a data base of this size will stand a very good chance of becoming a valid part of an ultimately more adequate general theory than we now have.

Now obviously it can be objected that the information in Appendix II is not necessarily correct, that Greenberg may have overlooked minority word order patterns co-existing with majority patterns, that he may have made the wrong choice about the basicness of one or other pattern in such cases, and so on. And to the extent that such inadequacies can be demonstrated, this may indeed affect the theoretical conclusions which I am going to derive from this data sample. But the possibility of error in data collections of this kind is an unavoidable fact of life. And what perturbs me more about current word order work is the sloppiness of the generalisations and of the theorising which are based upon Greenberg's data, rather than the reliability or otherwise of that data base itself. And particularly in the case of Appendix II it seems to me that the likelihood of serious error is small. It is not generally difficult to identify genitive and adjective expressions across languages and to assess their relative order to the noun. Adposition order is also generally fixed and transparent. The greatest variability occurs in the position of the verb relative to its arguments, though even here it is often (though by no means always) possible to set up one position of the verb as basic. And where languages depart from this basic position it is generally possible to specify which particular structures are variable relative to the verb (e.g. adverbials). Given these facts the possibility of serious classificatory error on Greenberg's part is minimised. Furthermore, I have argued in Hawkins (1976) and (in preparation) that the existence and nature of variant or doubling constructions in languages (NA & AN, VO & OV etc.) is largely predictable from the word order patterns of languages without such variation. This further reduces the significance of any classificatory error on Greenberg's part resulting from a failure to notice variant constructions in languages.

I am therefore going to assume that Appendix II is both a correct and a representative sample of word order co-occurrence possibilities exhibited by the world's languages. Not only that, I shall even assume that every single entry in the Appendix is significant and should not be dismissed. The alternative, eliminating some entries but not others, poses serious problems of justification. Thus, even if there is only a handful of languages, or just one language, in a particular co-occurrence cell, the significance of these entries should not be dismissed, because the co-occurrence type in question is actually attested, and because the

possibility of more such languages existing outside the sample is therefore real. Conversely, if there is no language in a co-occurrence cell I am going to assume that this co-occurrence type is universally impossible, unless there is internal evidence to the contrary within the Appendix itself. The total absence of a language type within so large a sample as Appendix II provides is therefore assumed to be principled. In addition, I am going to assume that the numbers of languages in the attested cells are not accidental. In short, I am simply going to describe Appendix II as it stands, and I am going to show that it contains patterns which have hitherto gone unnoticed, and which I can only describe as remarkable and even uncanny. The discovery of such patterns can only strengthen one's view of the reliability and representativeness of the data sample, on the assumption that universal word order patterns are indeed principled. By pointing out these patterns I hope to shed light on current unclarities and disagreements, and to crystallise future theorising and data-collection around certain key hypotheses.

Greenberg's (1963) Appendix II

Distribution of Basic Order Types:

1. VSO/Pr/NG/NA. Celtic languages; Hebrew, Aramaic, Arabic, Ancient Egyptian, Berber; Nandi, Masai, Lotuko, Turkana, Didinga; Polynesian languages and probably other Austronesian languages; Chinook, Tsimshian, Zapotec, Chinantec, Mixtec, and probably other Ote-Mangue languages.
2. VSO/Pr/NG/AN. Tagabili and probably other Philippine Austronesian languages; Kwakiutl, Quileute, Xinca.
3. VSO/Pr/GN/AN. Milpa, Alta, Nahuatl.
4. VSO/Pr/GN/AN. No examples.
5. VSO/Po/NG/NA. No examples.
6. VSO/Po/NG/AN. No examples.
7. VSO/Po/GN/AN. Papago.
8. VSO/Po/GN/NA. No examples.
9. SVO/Pr/NG/NA. Romance languages, Albanian, Modern Greek; West Atlantic languages, Yoruba, Edo group, most languages of Benue-Congo group including all Bantu languages; Shilluk, Acholi, Bari, most languages of Chad group of Hemito-Semitic but not Hausa, Neo-Syriac, Khasi, Nicobarese, Khmer. Vietnamese, all Thai languages except Khamti; many Austronesian languages including Malay; Subtiaba.
10. SVO/Pr/NG/AN. German, Dutch, Icelandic, Slavonic, Efik, Kredj, Maya, Papiamento.
11. SVO/Pr/GN/AN. Norwegian, Swedish, Danish.

12. SVO/Pr/GN/NA. Arapesh (New Guinea).
13. SVO/Po/NG/NA. No examples.
14. SVO/Po/NG/AN. Rutulian and other Daghestan languages in the Caucasus.
15. SVO/Po/GN/AN. Finnish, Estonian, Ijo, Chinese, Algonqian.
16. SVO/Po/GN/NA. Most Mandingo and Voltaic languages, Kru, Twi, Ga, Guang, Ewe, Nupe, Songhai, Tonkawa, Guarani.
17. SOV/Pr/NG/NA. Persian, Iraqw (Cushitic), Khamti (Thai), Akkadian.
18. SOV/Pr/NG/AN. No examples.
19. SOV/Pr/GN/AN. Amharic.
20. SOV/Pr/GN/NA. No examples.
21. SOV/Po/NG/NA. Sumerian, Elamite, Galla, Kanuri, Teda, Kamilaroi and other southeastern Australian languages.
22. SOV/Po/NG/AN. No examples.
23. SOV/Po/GN/AN. Hindi, Bengali, and other Aryan languages of India; Modern Armenian, Finno-Ugric except Finnish group; Altaic, Yukaghir, Paleo-Siberian, Korean, Ainu, Japanese, Gafat, Harari, Sidamo, Chamir, Bedauye, Nama Hottentot; Khinalug, Abkhaz and other Caucasian languages; Burushaski, Dravidian; Newari and other Sino-Tibetan languages; Marind-Anim, Navaho, Maidu, Quechua.
24. SOV/Po/GN/NA. Basque, Hurrian, Urartian, Nubian, Kunama, Fur, Sandawe, Burmese, Lushei, Classical Tibetan, Makasai, Bunak (Timor), Kate (New Guinea), most Australian languages, Haida, Tlingit, Zuni, Chitimacha, Tunica, Lenca, Matagalpa, Cuna, Chibcha, Warrau.

Section (A): The Theoretical Background: Greenberg and Vennemann

Current synchronic and diachronic research on word order has been most influenced by the work of Greenberg and Vennemann. I shall begin by briefly summarising their theories as a background against which to evaluate the significance of Appendix II.

Greenberg (1963) presents a total of thirteen implicational universals which explicitly relate verb position to other properties. They are formulated as follows (I shall retain Greenberg's numbering throughout):

(3) Languages with dominant VSO order are always prepositional.
(4) With overwhelmingly greater than chance frequency, languages with normal SOV order are postpositional.
(5) If a language has dominant SOV order and the genitive follows the governing noun, then the adjective likewise follows the noun.

52. The snow made us miss our train.
53. $_S$[it snowed] CAUSE $_S$[we missed our train]

It is also possible to represent motion in the same sort of way, although with a different predicate:

54. John went from Paris to Vienna.
55. $_S$[John AT Paris] BECOME $_S$[John AT Vienna][18]

I suggest that these representations may be significant because the ablative-expressed element is in the leftmost (antecedent[19]) S, i.e. the left-right order is here significant for the choice of case form[20]. This would suggest a principle of the type: if an element is in the antecedent S only, it will have an ablative case form; if it is in both S's it will be an instrumental. That works as a (very) informal statement as applied to the four case uses so far considered.

This is perhaps a rather unusual suggestion, but if we are serious about suggesting deep structures for sentences, we should be ready to accept any features of these structures as being potentially relevant in determining the form of surface structures. If left-right order (or something else equivalent to it) were to be significant in this way, it would be interesting confirmation of a feature of linguistic structures not usually accorded much attention.

Material and partitive case uses also have an ablative case form in Russian, these also being very common syncretisms, even in the small sample of languages I have looked at.

56. On sdelal stol iz dereva. "He made (the) table from wood".
57. Odin iz nix byl studentom. "One of them was (a) student".

(56) could presumably be derived as a causative sentence involving one thing becoming another, this fitting in with the sorts of structure I have sketched in (53) and (55). I have no very useful suggestions to make

[18] For the peculiar predicates see Kilby forthcoming and §9.2.

[19] After Nedjalkov & Sil'nickij 1969 p.6, I shall refer to the leftmost S as the antecedent S and the rightmost one as the consequent S.

[20] Of course it is not necessary that the order should be significant. If we had unordered arguments, then some device such as the numbering of arguments would be needed, but would serve the same purpose.

about the partitive, although it is clear that it has some features in common with quantification, and that the parallels with negative that I drew in §7.1 also hold here.

More details of my analysis of Russian cases can be found in a slightly different form in Kilby, forthcoming. Even from what I have said here, it is clear that much remains to be done to give a satisfactory account, but also that specific hypotheses as to the semantic structure of types of sentences do reveal interesting parallels which are at least suggestive as to the reasons for certain case forms encompassing the range of uses that they do.

8.2 The major case form in the relevant area in Twi is the alternating pair of verbs de and fa. Twi has a serial verb system, and virtually no nominal case morphology. It may be significant that the major correlates of the ablative in other languages are each separately expressed in Twi, the comparative by a verb meaning 'exceed', reason by a postposition nti, and partitive by a demonstrative (also postposed). All of the others may be realised as de/fa, usually translated as 'take' in English.[21,22]

There is no question of the same criteria being applied to the choice of forms in Twi as in Russian, as the cases are divided along rather different lines, but I would like to argue that the same general framework is capable of providing sets of criteria adequate to the characterisation of these uses as well. A clue to one possible approach can be found in sentences such as (58):

58. Me-de pensere too adaka no so. "I put the pencil on the box."
I-take pencil put box the top

This use of de is rather different from any of the others we have considered; a case grammarian would have no hesitation in putting pensere into the Objective (or nominative, depending on which case grammar) case, and this would be totally different from, say, Instrumental or Material case, as in (59) and (60):

[21] It is clearly no coincidence that most of the meanings of de/fa are more or less expressible by take in English:
i. prosecutive - he (over)took him on the inside
ii. instrumental - he took a stick and hit the elephant
iii. material - he took an egg and made an omelette
iv. comitative - he took his family to the zoo
v. travel - he took a bus to Euston
Although very approximate, this might suggest that the complete categorial separation of verbs and cases is somewhat artificial.

[22] This of course does not exclude the possibility of other means of expression for these case uses.

59. ɔ-de abaa bi boroo abɔfara no. "He beat the boy with a stick".
he take stick a beat boy the

60. ɔ-de nhoma bu kotoku. 'He makes a bag of leather'.
he take leather make bag

What appears to be common to all of these, however, is that they are all things that are at the receiving end of some action (as mentioned above in the case of the Russian instrumental) and yet at the same time in the antecedent S. For example, in (59) the stick is acted on (i.e. manipulated), but is part of the antecedent state. This appears to apply also with such cases as the comitative, at least in the examples I am aware of, such as (61):

61. ɔ-de ne yere kɔɔ Kumasi. 'He went to Kumasi with this wife'.
he take his wife went Kumasi

This approach would therefore seem to offer some possibility of giving a general characterisation of the de/fa forms within the same framework as my analysis of Russian cases, yet without claiming that precisely the same thing is happening in both languages. In fact, a framework such as the present one will obviously have a restricted number of possibilities, so that the same sort of case syncretisms will recur with some frequency; on the other hand, it potentially offers sufficient flexibility to characterise most of the relevant forms. One issue which I have not touched upon here, but which is going to be fairly crucial is that of whether the semantic structure of one language is the same as that of another language. An example of the sort of problem which might arise in this context is provided by sentence (58): there appears to be clear evidence in the surface form in Twi that my doing something to the pencil will be a constituent in a semantic representation of this sentence. There is no such clear evidence in Russian, as far as I know. It might always be possible to say that Russian has such a semantic representation for this sort of sentence, but that there are no surface reflexes of it. But the position that semantic representations must be universal is more a matter of belief than of proof at present, so that it may be preferable to leave this issue until there is some consensus as to what sort of evidence will have a bearing on it.

9.1 I am very far from thinking that this sketchy exemplification of a very tentative hypothesis can be regarded as substantial evidence for it; such evidence is likely to be possible only after much more detailed investigation of the case systems of many more languages than I have yet been able to do. I need to be able to show that all the uses of particular case forms in individual languages have a motivation in terms of the sorts of criteria that I have outlined. I also need to be able to extend the general framework to cover other case uses - i.e. those

outside the ablative-prosecutive 'field', and provide some account of the fact that some case uses appear in some sense transitional. For example, the agentive (e.g. in passive sentences) is frequently expressed either by an ablative or prosecutive case form (most familiarly in English, French, Russian, etc.), while in other languages (e.g. Evenki or Erz'a) it will be expressed as some sort of locative. I have no very convincing explanation for this, but clearly one is required.

I do, however, think that this discussion has shown two things: firstly that the range of devices available in providing a unified description for a case form is rather wider than is sometimes assumed. There may be a common regularity within the semantic representation of sentences involving particular case forms without it being possible to say that the various uses of these forms have some common meaning. Secondly, I think it is clear from these examples that the use of arbitrary features would simply obscure the regularities which appear to be at work: in order to obtain observational adequacy one would need a feature or combination of features to characterise every occurring combination of case uses within a single form. A brief perusal of the table in § 5.6 is sufficient to show that even for the small number of languages mentioned there, the set of features needed to characterise every class would be uneconomically large.[23]

9.2 Given the considerations I have mentioned, I think it is fairly clear that the sort of theories which would fulfil the requirements for a universal theory of case are not easily to be constructed. The only approaches I am aware of which would have the requisite flexibility are those of Hjelmslev (1935-7) and Jakobson (1936). There are, however, excellent reasons to avoid relying on theories such as these - the primary one being that they transfer much of the onus of definition from case meanings to abstract features such as 'coherence' or 'scope' (Umfang). Given enough ingenuity, it will presumably always be possible to construct an abstractsystem within which all occurring permutations are included (although even the problem of how to specify what is an occurring permutation is not without difficulty). If the application of the system is obscure enough, it will always be possible to dispute counterproposals on the grounds that they do not do justice to the original conception.

[23] This criticism would also apply to arbitrary category or case labels: e.g. there is a sense in which Anderson's use of the label 'abl' is arbitrary, not in terms of the forms it is intended to express, but in terms of the meaning that can be attached to it. It is hardly necessary to emphasise that the range of devices available to express arbitrary features of linguistic theories is vast and rapidly growing.

What I am aiming at, therefore, is a theory whereby independently justified linguistic units (in this case, probably semantic ones) will be sufficient to account for the distribution of case uses among a set of forms. I have argued elsewhere (Kilby 1973) that case grammars and generative semantics incorporating multiple higher predicates are merely 'notational variants'; it is for this reason that I have adopted representations which look rather like generative semantics trees, but which incorporate case-like predicates such as AT. I think that this sort of representation is capable of producing suggestive parallels within the semantic representations associated with a number of case forms.

Nonetheless, it must also be clear that what has in fact been achieved in this article is fundamentally the production of a set of data in search of a theory. I simply do not know why the partitive and the material case uses, for example, are related to the ablative in such a regular way in so many languages. Some of these parallels have been noted before, but the simple statement of identity of form can in no way be regarded as explanatory. But the nature of the data forces one to the conclusion that there has to be some link between them, whether it be semantic, syntactic, or something else as yet undreamt-of. If we understood the semantics and syntax of these constructions, we would no doubt be in a position to understand the parallels that occur between them. But conversely, if we can attempt to understand why some case uses are related, then the payoff in terms of our understanding of language in general could be very substantial.

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description of Russian, the only major exception being Jakobson (1936, 1958). And yet, at least as far as these particular uses are concerned, there appears to be clear cross-linguistic evidence that these same patterns occur many times independently. Only an approach to the uses of the instrumental which regards them as linked in a non-arbitrary way can be maintained if the cross-linguistic evidence is to be given any weight at all.

If we consider first of all the purely spatial uses - the ablative and the prosecutive - it is clear that the ablative is in some sense 'simpler' than the prosecutive in that it involves only motion from, whereas the prosecutive involves motion both to and from[17]:

50. On vyšel iz doma. "He went-out from (the) house".
51. On šel čerez dom. "He walked through (the) house".

This is incorporated, for instance, in Anderson's (1971) account of these case uses, the ablative being [abl], the prosecutive being $\begin{bmatrix} \text{abl} \\ \text{loc} \end{bmatrix}$. It seems to me that this same sort of distinction can plausibly be established, for instance, between the expressions of instrument and reason. Reason is the simpler notion in that it can be represented as an independent state or event which has a causal relationship to some other state or event. Instrument, on the other hand, is not independent, but something which is acted on as well as itself having an influence of some sort. It is thus a composite notion, like the prosecutive, while reason and ablative are both simple notions.

I think that this plausibly suggests a proportion:

ablative : prosecutive :: reason : instrument

This of course does not answer the question why spatial and non-spatial case uses should be similarly realised at all. I think that one line of investigation which is worth following up involves some structural similarities between the two types of relationship. It has been suggested variously (e.g. Nedjalkov & Sil'nickij 1969, Dowty 1972) that causative sentences can be represented as two sentences (or situations) linked by a causative predicate, e.g. (52) will be represented as (53):

[17] Bennett (1972) suggests that there should be a case named 'Path' which would express prosecutive meaning; while there is some evidence for this, it does not express the fact that 'Path' is a case of location and motion - in particular it would involve much redundancy in lexical specifications.

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(6) All languages with dominant VSO order have SVO as an alternative or as the only alternative basic order.

(7) If in a language with dominant SOV order there is no alternative basic order, or only OSV as the alternative, then all adverbial modifiers of the verb likewise precede the verb.

(10) Question particles or affixes specified in position by reference to a particular word in the sentence almost always follow that word. Such particles do not occur in languages with dominant order VSO.

(12) If a language has dominant order VSO in declarative sentences, it always puts interrogative words or phrases first in interrogative word questions; if it has dominant order SOV in declarative sentences, there is never such an invariant rule.

(13) If the nominal object always precedes the verb, then verb forms subordinate to the main verb also precede it.

(15) In expressions of volition and purpose, a subordinate verbal form always follows the main verb as the normal order except in those languages in which the nominal object always precedes the verb.

(16) In languages with dominant order VSO, an inflected auxiliary always precedes the main verb. In languages with dominant order SOV, an inflected auxiliary always follows the main verb.

(17) With overwhelmingly more than chance frequency, languages with dominant order VSO have the adjective after the noun.

(25) If the pronominal object follows the verb, so does the nominal object.

(41) If in a language the verb follows both the nominal subject and nominal object as the dominant order, the language almost always has a case system.

For many of the word order properties that are placed in relation to basic verb position in this way (adposition order, genitive order, etc.) Greenberg then gives ten additional implicational statements which either relate these non-verbal word orders to one another, or which relate them to altogether different word order patterns not mentioned in the above thirteen. The ten are: (2), (9), (18), (19), (20), (21), (22), (23), (24) and (40). Here are two representative examples:

(2) In languages with prepositions, the genitive almost always follows the governing noun, while in languages with postpositions it almost always precedes.

(18) When the descriptive adjective precedes the noun, the demonstrative and the numeral, with overwhelmingly more than chance frequency, do likewise.

The majority of Greenberg's implicational universals are presented as <u>non-statistical</u>. They are the following (where a universal embodies

two distinct claims I have distinguished them using a and b subscripts): (3), (5), (6), (7), (8), (11), (12), (13), (15), (16), (19b), (20), (21), (22a), (23a), (24), (25), (26), (27), (28), (29), (30), (31), (32), (33), (34), (35a), (36), (37), (38), (40), (43), (44), (45). The following are given as statistical: (2), (4), (9), (10), (17), (18), (19a), (22b), (23b), (35b), (39), (41). The only universals not included in these two lists are three non-implicational (i.e. unrestricted or absolute) universals: (1), (14), (42).

What emerges from Greenberg (1963) is, therefore, a set of word order correlations defined by implicational statements of a non-statistical or statistical kind. Such implicational statements are of the form: if a language has some property P, then it also has some other property Q. The antecedent P property in approximately half the word order universals involves verb position. The consequent Q properties in these cases are in turn either antecedent or consequent properties in further universals concerned with word order. As a result of this overlap between many universal implications, the position of the verb as implicational antecedent was seen as guaranteeing not only the co-occurrence of the consequent properties mentioned explicitly in the thirteen universals above ((3), (4), (5) etc.), but also the co-occurrence of such properties as are in turn dependent on these consequent properties (e.g. (2)). Greenberg recognised three basic positions of the verb: VSO, SVO and SOV, and in the 30-language sample 6 languages are VSO, 13 SVO and 11 SOV. Because of the implicational basicness of verb position, the labels VSO, SVO and SOV became used not only as descriptions for verb position as such, but as designators for whole language types, the correlating properties being precisely those mentioned in the various implicational universals.

The work of Vennemann is most succinctly summarised in Vennemann (1972). Vennemann makes three innovations. He first reduces Greenberg's threeway typology of VSO/SVO/SOV to a twoway typology: VO (or VX) versus OV (or XV). VSO and SVO are thereby both collapsed on the basis of the fact that they both have V before O. Secondly, Vennemann proposes a semantic-syntactic explanation for the co-occurrence universals which are presented merely as observational statements by Greenberg. Thirdly, he proposes a historical explanation for why languages change their typology over time, moving from OV to VO or vice versa. At the same time this theory accounts for why exceptions exist to his twoway typology: the relevant languages are moving historically towards a target type which is as yet only partially attained.

Vennemann summarises his OV/VO typology in the following diagram:

		Operator A	Operand B
I.	(a)	object	verb
	(b)	adverbial	verb
	(c)	main verb	auxiliary
	(d)	main verb	modal
	(e)	main verb	intensional verb
II.	(a)	adjective	noun
	(b)	relative clause	noun
	(c)	number marker	noun
	(d)	genitive	noun
	(e)	numeral	noun
	(f)	determiner	noun
III.	(a)	adjective stem	comparison marker
	(b)	standard	comparative adjective
	(c)	adverbial	adjective
IV.	(a)	noun phrase	relation marker (adposition i.e. postposition or preposition)
V.	(a)	indirect object	direct object
	(b)	temporal adverbial	directional adverbial

Languages with basic OV have co-occurring word order patterns in the order A + B, e.g. adverbial + verb, while VO languages have just the reverse, B + A, verb + adverbial, etc. There exist, in other words, just two language types exhibiting mirror-image contrasts for all the pairs of elements listed. The explanation which Vennemann proposes is that the categories in column A are all semantically operators on the corresponding operand categories under B. The criteria for allocating some category to either operator or operand status are that

> "semantically the application of an operator results in a specification of the operand predicate, and, syntactically, that the application of an operator to an operand results in a constituent of the same general syntactic category as that of the operand ..." (Vennemann (1972))

For example,

> "Adverbials are operators on verbs because the result of the operation is a complex verb rather than a complex adverbial. Main verbs which are infinitives are operators on finite modals, which are finite verbs, because the result of the operation is a finite verb rather than an infinitive. And noun phrases are operators on prepositions, which are transitive adverbials, because the result of the operation is a prepositional phrase, i.e. an adverbial, rather than a noun phrase". (Vennemann (1972))

Having classified these word order categories as either operators or operands, Vennemann then formulates his "principle of natural serialisation". According to this principle languages will serialise their operator-operand pairs either from left to right (OV or A + B languages), or from right to left (VO or B + A languages). The principle is stated as follows:

$$\left\{ \text{Operator} \quad \{\text{Operand}\} \right\} \Rightarrow \left\{ \begin{array}{ll} [\text{Operator} & [\text{Operand}] \\ [[\text{Operand}] & \text{Operator}] \end{array} \right\} \quad \begin{array}{l} \text{in OV languages} \\ \text{in VO languages} \end{array}$$

Vennemann therefore defines his two language types abstractly. He notes that not all languages are fully consistent with his schema, and hypothesises that this is for historical reasons. The inconsistent languages are those which are moving from one type to the other. He offers a phonological and morphological explanation for why V should shift position relative to O, and he then proposes the natural serialisation principle as the source of an analogical pull which brings about the subsequent acquisition of the other operator-operand orderings which are harmonic with VO or OV respectively.

Section (B): Four Preliminary Observations

Turn now to Appendix II. Although the overall impression appears to be unstructured, there are four initial observations of potential significance that can be made.

The first concerns the fact that a total of eight of the twenty-four cells have no entries. In other words, one third of the logically possible co-occurrences of these four properties are unattested in any language. Given the size of Greenberg's data base, I regard the total absence of such languages as very significant, and in need of explanation.

The second observation concerns the distribution of languages across

the sixteen types that are attested. This distribution varies. Four cells are very large: 1, 9, 23 and 24; four are medium-sized: 2, 10, 16 and 21; five have between three and five languages only: 3, 11, 14, 15 and 17; and three have only single exemplifications: 7, 12 and 19. It would clearly be desirable if we could account for this distribution in some principled way. Given the size of the sample I assume again that it is not accidental.

A third preliminary observation concerns the status of SVO languages. These languages differ from VSO and SOV languages in a number of ways. Only one of the eight unattested cells falls within the SVO block (13), although SVO languages account for one third of the total number of logical co-occurrence possibilities. VSO languages contain four cells without exemplification, and SOV languages three. By contrast, SVO languages contain proportionately more of the smaller groups. Two of the four medium-sized cells (10 and 16) are SVO; three of the five types containing 3-5 languages (11, 14 and 15) are SVO, as is one of the three types with a unique exemplification (12). SVO languages are also intermediate between VSO and SOV in some respects. Although most have prepositions (9, 10, 11 and 12), very many have postpositions (14, 15 and 16). All but one VSO language have prepositions, and all but five SOV languages have postpositions. Whereas VSO and SOV therefore show a marked preference for one adposition order over the other, SVO has a significant co-occurrence of both. Notice in addition that noun-modifier co-occurrences which are totally unattested in VSO and SOV languages are attested in SVO languages. Thus, no VSO language has a co-occurrence of GN & NA (4 and 8):

4. VSO/Pr/GN/NA No examples
8. VSO/Po/GN/NA No examples

and no SOV language has the co-occurrence NG & AN (18 and 22):

18. SOV/Pr/NG/AN No examples
22. SOV/Po/NG/AN No examples

Yet both these co-occurrence sets are attested in SVO languages (12 and 16, and 10 and 14 respectively):

12. SVO/Pr/GN/NA Attested
16. SVO/Po/GN/NA Attested

10. SVO/Pr/NG/AN Attested
14. SVO/Po/NG/AN Attested

SVO languages therefore seem to have properties of both VSO and SOV. If, following Greenberg, we are to regard languages with SVO as differing from VSO and SOV not only in basic verb position, but also in general

type (as reflected in the word order patterns which are claimed to correlate with this position of the verb) then it is clear that SVO is a separate type by default. It differs from VSO and SOV by combining properties of both, and by not having regular and characteristic properties of its own. Such a conclusion is also supported by Greenberg's thirteen implicational universals referring to basic verb position. Notice that all of these mention either VSO or SOV, never SVO, except for one ((6)) which claims that SVO is an alternative structure available to VSO languages. But even here, the antecedent property P which is claimed to determine the co-occurrence of the consequent property Q, is again VSO. In none of Greenberg's universals is SVO itself an implicationally antecedent property, and hence nothing is being claimed to correlate with SVO in a unique and principled way. SVO is therefore not a type but a rag-bag which contains properties of both VSO and SOV languages, and none of its own.

The fourth preliminary observation is slightly more subtle. Consider first SOV languages. We have seen that none of them allow the co-occurrence NG & AN (18 and 22), whether they are prepositional or postpositional. Now what could explain this absence? It cannot be that any one of these noun-modifiers taken on its own is incompatible with SOV, because we find NG occurring in a reasonable number of attested SOV languages (17 and 21)

17. SOV/Pr/NG/NA Attested
21. SOV/Po/NG/NA Attested

and we also find AN occurring in a vast number of SOV languages (19 and 23)

19. SOV/Pr/GN/AN Attested
23. SOV/Po/GN/AN Attested

So there must evidently be something about NG & AN co-existing together which explains the total absence of this co-occurrence in SOV languages. Notice also the complete absence of the GN & NA co-occurrence in VSO languages (4 and 8). Again GN as such is attested with VSO (3 and 7):

3. VSO/Pr/GN/AN Attested
7. VSO/Po/GN/AN Attested

as is NA (1):

1. VSO/Pr/NG/NA Attested

so it must be something about GN & NA together which prevents them from co-occurring in VSO languages.

Now, we could try to account for these facts using some form of

statistical statement. We might observe that SOV languages do not in general have NG, that VSO languages do not have GN in general, and that the unattested pairs, 18 and 22, and 4 and 8, were therefore unattested precisely because of the statistical aversion of NG and GN to co-occur with SOV and VSO respectively. But such statements miss the point, in several important respects. For some SOV languages do have NG, and some VSO languages do have GN. But what absolutely none of them have is the co-occurrence of NG & AN and GN & NA respectively. I infer from this that when several properties are taken together, and when, for example, both genitive and adjective order are placed in relation to verb position, it becomes possible to achieve exceptionless correlations which are impossible to state when only single properties (e.g. genitive or adjective order on their own) are related to one another. And notice two other regularities in these examples. Of the four logically possible co-occurrences of genitive and adjective order:

NG & NA
NG & AN
GN & AN
GN & NA

both VSO and SOV languages have exemplifications for three only, and no examples of a fourth, as follows:

VSO		SOV	
NG & NA	Attested	NG & NA	Attested
NG & AN	Attested	* NG & AN	No examples
GN & AN	Attested	GN & AN	Attested
* GN & NA	No examples	GN & NA	Attested

and the co-occurrence which is excluded with VSO (GN & NA) is the exact mirror-image of the unattested SOV co-occurrence (NG & AN). If these facts are accidental, I am going to eat my hat.

Section (C): The Empirical Weaknesses of Greenberg and Vennemann

Given a corpus of data such as Appendix II, we must attempt to discover whatever regularities we can, and to formulate them in the most adequate way possible. Descriptive statements must be neither too general nor too restrictive, just like the rules of any single grammar, but should account exactly for the data to hand. Let us see how the empirical statements of Greenberg and Vennemann fare as descriptions of the data.

Greenberg proposes five statements that are intended to cover Appendix II as well as the 30-language sample. They are the implicational

universals (2), (3), (4), (5) and (17), which I repeat for convenience (dividing universal (2) now into two parts):

(2a) In languages with prepositions, the genitive almost always follows the governing noun.

(2b) In languages with postpositions, the genitive almost always precedes the governing noun.

(3) Languages with dominant VSO order are always prepositional.

(4) With overwhelmingly greater than chance frequency, languages with normal SOV order are postpositional.

(5) If a language has dominant SOV order and the genitive follows the governing noun, then the adjective likewise follows the noun.

(17) With overwhelmingly more than chance frequency, languages with dominant order VSO have the adjective after the noun.

Of these five universals, two are presented as exceptionless ((3) and (5)), and the other three are statistical. In fact, Greenberg mentions an exception to universal (3) in an additional note (p.107) which was pointed out to him after completion of the paper and which I have inserted into the Appendix as reproduced here. But let us forget about this language (Papago, type 7) for the time being and assess Greenberg's statements against the body of data that they were originally intended to cover.

There are nine totally unattested cells in Greenberg's original data base: 4, 5, 6, 7, 8, 13, 18, 20, 22. Of these he rules out six in a principled way. The non-statistical universal (3) accounts for the absence of 5, 6, 7 and 8, all of which have postpositions:

5.	VSO/Po/NG/NA	No examples
6.	VSO/Po/NG/AN	No examples
7.	VSO/Po/GN/AN	No examples
8.	VSO/Po/GN/NA	No examples

And the non-statistical (5) rules out 18 and 22:

18.	SOV/Pr/NG/AN	No examples
22.	SOV/Po/NG/AN	No examples

This leaves three unattested cells which are not accounted for:

4.	VSO/Pr/GN/NA	No examples
13.	SVO/Po/NG/NA	No examples
20.	SOV/Pr/GN/NA	No examples

Now each of these three violates at least one of the statistical implications. Co-occurrence type 4 violates (2a) and (2b), as does 13. And

type 20 violates both (2a), (2b) and (4). But these statements are not sufficient to account for the total absence of exemplifications for types 4, 13 and 20, because they are merely statistical statements which allow explicitly for exceptions, and which, as we shall see, do have very many exceptions. As a result, these three unattested types are treated very differently from the other six, which are totally ruled out. And the implicit claim is made that languages of types 4, 13 and 20 might exist, albeit as a minority. But what evidence is there in the Appendix for such an assumption? Absolutely none, in fact. All that we have is nine unattested types. And barring evidence to the contrary, we must assign them all the same status. If we are to totally exclude one, we must totally exclude them all. If statistical laws are to account for one, such laws should account for them all. There are, in other words, no principled grounds for ruling out six of the nine in toto, and the other three statistically, and Greenberg is being both inconsistent, and is failing to account adequately for three of these nine types given his account of the other six. His different treatment of the two groups seems to be based instead on descriptive convenience. He evidently wishes to rule out at least two co-occurrence types at a time with each exceptionless statement. Universal (3) rules out four types, and universal (5) two. Apparently he can find no additional unattested type to which 4, 13 and 20 could be related so that at least two types could be excluded in a non-statistical statement. But although one can sympathise with this search for generalisations, it does not justify the differential status that is assigned to the unattested types, with the resulting different predictions that are being made about languages outside the data sample.

Notice secondly that Greenberg could have excluded type 4 in a principled statement which excluded at least one other unattested type if he had noticed the interesting parallelism between VSO and SOV languages with regard to their noun-modifier co-occurrences (our fourth preliminary observation, section (B)). Thus, corresponding to (5), which excludes 18 and 22, he could have set up the following counterpart in order to exclude both 4 and 8:

(46) If a language has dominant VSO order and the genitive precedes the governing noun, then the adjective likewise precedes the noun.

4.	VSO/Pr/GN/NA	No examples
8.	VSO/Po/GN/NA	No examples

The third weakness of Greenberg's statements concerns the differences in the distribution of languages across the attested types. Greenberg offers no principled account of these differences, and hence the implicit claim is made that they are accidental. However, Greenberg does, indirectly, make some limited claims about distribution. If a language

type is an exception to some statistical universal, it is thereby claimed to be a minority type. Thus, language type 3

3. VSO/Pr/GN/AN Milpa, Alta, Nahuatl

is an exception to universal (2a) linking prepositions to NG, and so this type is defined into minority status. But no explanation is offered for the difference between the four very large and the four medium-sized cells, and nor is there any exact correlation between exceptional language type, and distributional size. The exceptions range from medium-sized (i.e. 5+) cells to single-language cells.

Our fourth comment on Greenberg concerns again his statistical statements. Some 20% of the named languages in Appendix II are exceptions to at least one of the statistical statements given at the beginning of this section. What kind of reality are we dealing with, therefore, in statements which account for 80% of the data? I believe that Greenberg is getting at something real in these statements, but I am going to argue that we need to make a systematic distinction between two different types of word order universals, and that Greenberg has confused the two. On the one hand, we need to set up genuine universal constraints on linguistic structure which distinguish co-occurring from non-co-occurring word order patterns in a principled way. Where these constraints are of the implicational kind they have a logical structure of the form: if a language has some property or properties P then it also has some other property or properties Q. Such statements may conceivably have some exceptions, but their value obviously decreases with each exception that is found. On the other hand, I am going to present evidence for the existence of a distributional generalisation which accounts for all the relative numbers of languages in the co-occurrence cells which are actually attested, but which is not capable of distinguishing the attested from the non-attested types. The logical structure of this generalisation is as follows: given any two sets of languages of certain specified types, A and B, the quantity of languages of type A will exceed that of type B, in accordance with some formula F. Again, there may conceivably be exceptions to the quantitative predictions of this generalisation, but as before its value decreases with every set of exceptions that can be found.

It transpires that Greenberg's statistical statements are covering, in an oblique way, some of the output predictions of the distributional generalisation, by defining high and low frequency word order co-occurrence types. But Greenberg's statements are statistical, not because the distributional facts about the attested word order co-occurrences are merely approximate and only 80% principled, but simply because he fails to capture the significant generalisations covering distribution that can be made. And in general, the existence of exceptions is detrimental to (though compatible with) both the universals defining possible and

impossible co-occurrence types, and those defining quantities of attested types.

Let us enumerate the exceptions to Greenberg's statistical implications. Universal (2a) disallows the co-occurrence types: 3, 4, 11, 12, 19, 20. Of these the following are exceptions (the numbers of attested languages of each type are placed in parentheses): 3 (three lgs), 11 (three lgs), 12 (one lg), 19 (one lg), i.e. eight exceptional languages in all. Universal (2b) disallows: 5, 6, 13, 14, 21, of which the following present exceptions: 14 (one lg + some Daghestan lgs), 21 (six lgs + some SE Australian lgs), making seven named exceptional languages, plus the two groups in question. Universal (4) disallows: 17, 18, 19, 20. The exceptions are: 17 (four lgs), 19 (one lg), making five languages in all. Universal (17) disallows: 2, 3, 6, 7. The exceptions are: 2 (four lgs + various Philippine Austronesian lgs), 3 (three lgs), 7 (one lg), i.e. eight languages plus the Philippine languages. And finally, universal (3) has the one exception, Papago, type 7.

Adding up these figures we discover that twenty-four named languages, plus three groups of languages, are exceptions to one of the statistical universals, and that five named languages are exceptions to two. Out of a total of 122 named languages, twenty-four, or 19.7% are an exception to at least one implicational universal, therefore. 80.3% accuracy is not bad, but it is not very good either.

The fifth weakness in Greenberg's description of the data concerns the status of SVO languages. To refer to these languages as a type, along with VSO and SOV, as Greenberg does, is to make, at least implicitly, the claim that other word order patterns correlate with SVO just as they do with VSO and SOV. But we have already seen that nothing correlates in a principled way with SVO, neither in the 30-language sample, nor in Appendix II. Hence, SVO is not a significant type. Why then did Greenberg propose a three-way typology which included SVO, an order upon which absolutely nothing is implicationally dependent? Evidently because VSO and SOV did guarantee implicational correlations with other word orders, even though SVO did not. And since he wished to propose VSO and SOV as a type, the other common verb position, SVO, became elevated to type status by default. But to propose a type which has no type characteristics is a rather strange thing. If this is a necessary consequence of using verb position to classify the world's languages, it raises the question of whether the whole verb position typology is correct. Such doubts are amply nourished by a reconsideration of the thirteen VSO and SOV universals ((3), (4), (5) etc.). Five of these thirteen admit of exceptions ((3), (4), (10), (17) and (41)); i.e. 38.5% are statistical, whereas the total number of statistical implicational statements given by Greenberg as a whole comes to thirteen out of forty-six, or only 28.3%. (I am including (3) as statistical and (22a) and (22b) etc. as separate

statements in these figures). Thus, the VSO and SOV universals account for a higher proportion of the exception-ridden universals than they should.

We have presented a total of five empirical weaknesses in Greenberg's implicational statements covering Appendix II. They can be summarised as follows:

1) the failure to rule out three unattested co-occurrence types in a principled way;

2) the failure to notice that the total absence of NG & AN in SOV languages is matched by the total absence of GN & NA in VSO languages;

3) the failure to capture distributional generalisations;

4) some 20% of the named languages are exceptions to at least one universal implication,

5) SVO is not a significant type.

Let us consider now Vennemann's predictions about the data in Appendix II, as embodied in his natural serialisation principle (p.97). First of all, Vennemann does not give a fully accurate account of Greenberg's work. He writes:

> "he (i.e. Greenberg) relates positional properties of languages to each other in implicational statements of the character 'if a language has the property a, then with more than chance frequency it also has the property b' ". (Vennemann (1973))

and

> "his (i.e. Greenberg's) universals are, for the most part, implicative statements based on statistical data; they take the form 'If a language has A, it also has B with more than chance frequency' ". (Vennemann (1974))

In fact, most of Greenberg's implicational universals are presented as non-statistical statements, as we have seen. Even with the universals referring to word order, which account for a higher proportion of the statistical statements than the others, a clear majority are still presented as non-statistical. Eight of the thirteen defined on VSO and SOV are non-statistical ((5), (6), (7), (10), (12), (13), (15), (16) and (25)). With the additional ten statements referring to non-verbal word orders, three embody two statements ((19), (22), (23)), making thirteen statements in all. Of these thirteen seven are non-statistical ((19b), (20), (21), (22a), (23a), (24) and (40)). Thus, a total of fifteen out of twenty-six word

order statements are non-statistical (or 57.7%), while eleven out of twenty-six are statistical (or 42.3%). Although this balance is close, such figures scarcely justify the impression given by Vennemann that Greenberg's universals are predominantly statistical. They are not. Only 42.3% of the word order universals are statistical, and only 28.3% of the implicational universals as a whole. It could be, of course, that Vennemann knows of further exceptions to Greenberg's implicational universals. But if he does we are not informed of them. And in any case, Vennemann's quotes give a false summary of what Greenberg actually proposed, whether Greenberg is right or not.

Let us now enumerate the empirical predictions of the natural serialisation principle. There are in fact huge numbers of exceptions. Taken literally, the principle predicts that only three of the twenty-four co-occurrence types in Appendix II have consistent operator-operand orderings, 1, 19 and 23. All of the others involve at least one ordering violation. The principle therefore correctly predicts the existence of languages of type 1, 9 and 23: and it correctly predicts the impossibility of the eight unattested types, 4, 5, 6, 8, 13, 18, 20, 22. But the following types are exceptions to it as it stands: 2 (four lgs + other Philippine Austronesian lgs), 3 (three lgs), 7 (one lg), 10 (eight lgs), 11 (three lgs), 12 (one lg), 14 (one lg + other Daghestan lgs), 15 (five lgs), 16 (nine lgs + most Mandingo and Voltaic lgs), 17 (four lgs), 19 (one lg), 21 (six lgs + other SE Australian lgs), 24 (twenty-three lgs + most Australian lgs). If we add up these figures we discover that a total of 69 named languages out of 122 are exceptions (or 56.6%), as are five whole groups of languages ranging from Philippine Austronesian, Daghestan, Mandingo and Voltaic, SE Australian, and most other Australian languages. Given figures such as these, one wonders whether the natural serialisation principle is really so natural after all.

Admittedly, Vennemann is talking in terms of mere tendencies to co-occur (mistakenly tracing his pedigree back to Greenberg), and is defining two ideal word order types from which languages may deviate. But what is the status of a typological principle which accounts for only 43.4% of the data correctly? One could think of any number of ideal types with this degree of empirical support. Not only is the existence of 56.6% exceptions unpalatable for any theory, but there are, as we have seen, eight types out of the twenty-four which have no exemplifications in any natural language, and a further three which have only one attested language. The non-existence of such languages is evidently no accident, so why does Vennemann wish to convert all of Greenberg's carefully worded statements (which attempt at least to account for non-existent and minority types) into mere co-occurrence tendencies? Not only do such loose tendencies have very many exceptions, they assign to a type such as 24, which is among the four largest in the Appendix, the same

deviance (one serialisation violation: NA) as, for example, 22 (cf the deviant NG) and 4 (cf the deviant GN), both of which are totally unattested. Vennemann may have proposed an explanatory principle, therefore, but it is at the expense of observational adequacy, which can only have consequences for the validity of the serialisation explanation itself. Nor is it the case that Vennemann's historical theory of word order change provides a principled explanation for the existence of inconsistent languages, as Vennemann claims. For his historical theory is both self-contradictory and empirically unsupported, as we argue in section (F).

The first and major problem for Vennemann's natural serialisation principle as it stands is therefore the complete absence of empirical support. At the same time we notice, secondly, that one third of Greenberg's co-occurrence types are unattested, and hence compatible with exceptionless universals (as in most of Greenberg's statements) rather than with loose tendency-based statements.

Notice thirdly that there are regularities within the many exceptions to the natural serialisation principle which Vennemann is not capturing. For example, we have seen that VSO languages have a total aversion to the co-occurrence GN & NA (4 and 8), and SOV languages to the co-occurrence NG & AN (18 and 22). Within Greenberg's framework, it is possible to capture exceptionless observational generalisations for both these unattested pairs (even though Greenberg only accounted for one of them). But in Vennemann's format there is no way of distinguishing VSO & GN & NA (4 and 8) from either VSO & NG & AN (2) or VSO & GN & AN (3 and 7), both of which are attested, for all these noun-modifier co-occurrences are deviant according to the serialisation principle, which requires that VSO languages should have only NG & NA. The same problem arises for SOV languages. Two of the attested types (SOV & GN & NA (24) and SOV & NG & NA (17 and 21)) are as deviant as the unattested SOV & NG & AN (18 and 22). Thus, Vennemann's two-way operator+operand/operand+operator system is failing to capture observational generalisations. There are principles within his exceptions which are going unnoticed.

Nor is there any consistent correlation between the number of operator-operand serialisation violations within any co-occurrence type and the presence or absence of attested languages. Thus, SOV languages which have NG & NA have two noun-modifier discrepancies with SOV, and yet there are ten named languages + other SE Australian languages of this type (17 and 21), whereas there are no SOV languages at all with just one serialisation violation of the form SOV & NG & AN (18 and 22), and so on throughout the Appendix.

The fourth objection that can be made to Vennemann is similar to the

third weakness of Greenberg's statements. There is no principled account of the distributional differences between the attested language types. Again, minority status does not correlate with exceptional status since the exceptions range from type 24 (with twenty-three named languages + most Australian languages) to types with only one attested language. No predictions are therefore made concerning the different sizes of the cells.

Fifthly, the problems with the status of SVO languages carry over to Vennemann's theory. Vennemann collapses VSO and SVO into one type, VO. But we have seen that SVO is a mixed type, having properties of both VSO and SOV, and none of its own. SVO can therefore not be collapsed with either VSO or SOV. It is true that SVO does share with VSO the feature V before O, and most SVO languages agree with VSO languages in having, for example, prepositions rather than postpositions. But these facts are not sufficient. There is still a large number of SVO languages which have postpositions, namely fifteen out of forty-one named languages (or 36.6%). In addition, ten SVO languages (24.4%) have the co-occurrence GN & NA (types 12 and 16), which is not found in any VSO language (4 and 8). One cannot simply ignore the 36.6% SVO languages with postpositions, and the 24.4% SVO languages with GN & NA, and lump SVO together with VSO in this way. To do so is to destroy any implicational validity which VSO has by creating a single type with poor correlations, whereas previously there was at least one valid type, VSO, and one invalid one, SVO. The absence of correlations with SVO therefore carries over to the new supposedly uniform type, and undermines the significance both of VO, and of the whole VO/OV dichotomy in consequence. The collapsing of the typologically irrelevant SVO with VSO therefore creates as many problems as it solves, and suggests yet again that this kind of verb-based typology is the wrong way to go. Something much more radical is required.

We have discussed five empirical weaknesses in the predictions which Vennemann's natural serialisation principle makes concerning the data of Appendix II. These were:

1) the misrepresentation of the balance between statistical and non-statistical statements in Greenberg;

2) the existence of vast numbers of exceptions;

3) the failure to notice patterns within the exceptions (distinguishing attested from non-attested types);

4) the failure to account for distributional differences;

5) the collapsing of VSO and SVO into a common type, even though SVO is typologically ambivalent.

I think the point has been made that current universal statements by both Greenberg and Vennemann do not succeed in capturing adequate empirical generalisations covering the data of Appendix II. In the next two sections I shall attempt some improved generalisations. I shall argue in section (D) that the attested and non-attested co-occurrence types can be most adequately described in terms of six implicational statements operating in conjunction with one another. In section (E) I shall propose a single distributional generalisation which predicts the quantitative dif'erences between the attested cc-occurrence cells.

Section (D): Six Implicational Generalisations in Appendix II

First of all, what exactly does it mean to say that if a language has some property P, then it also has some other property Q? What is an implicational statement claiming, and what kind of language typology is thereby being defined into existence?

The form of Greenberg's implicational universals is based on statements using 'if-then' in everyday reasoning, for example 'if Harry reads to me this evening then I will be happy' or 'if Fred was eating an apple then he must have been eating a fruit'. Such 'if P then Q' statements seem straightforward enough on the surface, but there is actually more to them than meets the eye. By way of illustration, let P and Q have the following values:

P Fred was eating an apple
Q Fred was eating a fruit

Obviously, if P is true, and Fred was eating an apple, then Fred must have been eating a fruit, and hence Q must also be true. But there is nothing about an 'if P then Q' statement which requires that P is actually true. So what is the status of Q in the event that P is false, i.e. in the event that Fred was not eating an apple? There are in fact two possibilities. Either Fred may still have been eating a fruit, even though it did not happen to be an apple, in which case Q is still true even though P is false. Or Fred may not have been eating any fruit at all, in which case both P and Q are false. To claim 'if P then Q' in this case is therefore not to make any definite commitment about the truth of either of these two propositions, but simply to assert that if P is true then Q is also true, and if P is false then Q may be either true or false. What is disallowed by 'if P then Q' is simply, and uniquely, that P could be true and yet Q false, i.e. that Fred could be eating an apple and yet not eating a fruit.

These semantic intuitions that we have about 'if P then Q' statements are formalised in a clear and intuitive way in treatments of implicational relationships between propositions within symbolic logic. The truth table for the 'if-then' operator, often represented as $\rightarrow$ is as follows:

P	$\rightarrow$	Q
T	T	T
T	F	F
F	T	T
F	T	F

The overall value true, T (centre column), is assigned to the implicational statement when both P and Q are true, when P is false and Q true, and finally when both P and Q are false. But the overall value false, F, is assigned in the event that P is true and Q false. Thus, three assignments of the values true and false to P and Q are compatible with the validity of the implication, $P \rightarrow Q$, and just one assignment is not. In effect, the implication $P \rightarrow Q$ defines three possible states that the world can be in, and excludes a fourth, as follows:

i/ P & Q (i.e. P=T and Q=T)
ii/ -P & Q (i.e. P=F and Q=T)
iii/ -P &-Q (i.e. P=F and Q=F)
• iv/ P &-Q (i.e. P=T and Q=F)

Either (P & Q) Fred was eating an apple and was therefore eating a fruit. Or (-P & Q) Fred was not eating an apple though he was still eating a fruit. Or (-P &-Q) Fred was neither eating an apple nor eating a fruit. But what is excluded is the combination P &-Q, whereby Fred would be eating an apple and yet not eating a fruit.

This kind of tabulation makes transparent the fact that such implicational statements as 'if Fred was eating an apple then Fred was eating a fruit' are not reversible. Although the truth of P guarantees the truth of Q (cf lines i/ and iv/), the truth of Q does not guarantee the truth of P, since the implication is valid both when P & Q and when -P & Q hold (cf lines i/ and ii/). However, where P and Q stand for the propositions:

P Fred is a bachelor
Q Fred is an unmarried man

the implication does go in both directions. From P we can infer Q, and from Q, P. This bilateral implication therefore defines two possible states that the world can be in, and excludes two:

i/ P & Q
iii/ -P &-Q

* ii/ -P & Q
* iv/ P &-Q

Either Fred is both a bachelor and an unmarried man (P & Q), or he is neither (-P &-Q). The mixed positive and negative values of P and Q are both ruled out.

Returning to the non-reversible, or unilateral, implications, notice now the consequences of employing such statements within the context of universal grammar. To claim 'if P then Q' where P and Q stand for linguistic properties such as:

P a given language has basic VSO order
Q that language has prepositions

is first of all to allow for the possibility that some languages may not have either of these two properties. This possibility distinguishes implicational universals from "absolute" or "unrestricted" universals, in which the properties in question must be exemplified in and must hold for every single language of the world. Secondly, whereas an absolute universal predicates generally a single property of all the world's languages, implicational universals define the necessary co-occurrence of at least two properties in specified languages. Thirdly, an 'if P then Q' implicational universal does more than state that the properties P and Q co-occur. It defines three possible co-occurrences that languages can have, and excludes a fourth. Hence, to claim that if a language has VSO (P), then it has prepositions (Q), is to allow for the following co-occurrence types:

i/	P & Q	VSO & Prepositions
ii/	-P & Q	-VSO & Prepositions
iii/	-P &-Q	-VSO &-Prepositions
* iv/	P &-Q	VSO &-Prepositions

The negative property -VSO can be realised by either SVO or SOV in Greenberg's system, whereas the negative -Prepositions has just a unique value, Postpositions. This table can therefore be more revealingly represented as:

i/	P & Q	VSO & Prepositions
ii/	-P & Q	(SVO v SOV) & Prepositions
iii/	-P &-Q	(SVO v SOV) & Postpositions
* iv/	P &-Q	VSO & Postpositions

An implicational universal thus necessarily embodies a typological claim about co-occurring and non-co-occurring linguistic properties. The

statement 'if P then Q', if valid, defines into existence three language types: P & Q, -P & Q and -P &-Q; and excludes a fourth: P &-Q.

Where negative values of P and Q have more than one realisation, the effect is to multiply the co-occurrence possibilities. Thus, the above universal actually allows for five co-occurrence possibilities, all of which are in fact attested in Appendix II:

i/	P & Q	VSO & Prep	(1, 2, 3)
ii/	-P & Q	a) SVO & Prep	(9, 10, 11, 12)
		b) SOV & Prep	(17, 19)
iii/	-P &-Q	a) SVO & Postp	(14, 15, 16)
		b) SOV & Postp	(21, 23, 24)
* iv/	P &-Q	VSO & Postp	(7, one exception)

The implicational universal which we have just illustrated, (Greenberg's universal (3)) is therefore reasonably accurate. All the co-occurrences which are claimed to exist do so, and there is only one single exception of the form P &-Q. However, all too often universal implications are proposed in the literature which do not test explicitly for all co-occurrence types. In particular there is an almost systematic failure to check whether -P & Q and -P &-Q are both attested. Imagine, for example, that it was discovered that the property -P was always accompanied by -Q, and never by Q, and that P & Q was also a valid co-occurrence. In such a case we would have two attested types:

i/ P & Q
iii/ -P &-Q

and two that were unattested:

* ii/ -P & Q
* iv/ P &-Q

The resulting typology of languages is clearly very different, and would require on this occasion a bilateral implicational statement holding between P and Q, rather than a unilateral one.

Before we turn to Appendix II again, we must ask: what kind of reality are these unilateral implicational statements capturing such that just three of four logical possibilities are being allowed? And what kind of explanation are we going to want to propose for these observational generalisations? The crucial factor here is the validity and existence of the second co-occurrence -P & Q. Evidently, languages can have Q without having P, in addition to having both or neither. In effect, Q is a prerequisite for P. Only in those languages which already have Q do we find P, and in the event that Q is absent, P is necessarily absent. In

searching for explanations we must therefore try to find something basic about Q which allows a language to have P as well, and which prevents a language from having P in the event that Q is absent. Notice in this regard the logical equivalence of $P \rightarrow Q$ to $-Q \rightarrow -P$ (e.g. if Fred was not eating a fruit, then Fred was not eating an apple).

I shall now propose six implicational statements which will between them distinguish the attested from the non-attested co-occurrence types of Appendix II. Consider first types 1 through 4:

1. VSO/Pr/NG/NA Attested
2. VSO/Pr/NG/AN Attested
3. VSO/Pr/GN/AN Attested
4. VSO/Pr/GN/NA No examples

Three types are attested and one is not. This is exactly the number of possible types that is defined by a unilateral implication. The variant orders here involve the noun-modifiers only. Leaving aside VSO and Prep for the moment notice that the following implication defines the three possible noun-modifier co-occurrences and rules out the fourth:

a) If the order of noun and adjective is NA (P), then the order of noun and genitive is NG (Q), i.e. $NA \rightarrow NG$

i/	P & Q	NA & NG	(1)
ii/	-P & Q	AN & NG	(2)
iii/	-P & -Q	AN & GN	(3)
* iv/	P & -Q	NA & GN	(4 - no examples)

This implication is logically equivalent to a $-Q \rightarrow -P$ version, namely b):

b) If the order of noun and genitive is GN (P), then the order of adjective and noun is - AN (Q), i.e. $GN \rightarrow AN$

i/	P & Q	GN & AN	(3)
ii/	-P & Q	NG & AN	(2)
iii/	-P & -Q	NG & NA	(1)
* iv/	P & -Q	GN & NA	(4 - no examples)

These formulations define the same range of data, so how can we choose between them?

The b) version is in fact an unnatural statement with which to capture these noun-modifier co-occurrence possibilities. Why? Because the properties which are explicitly mentioned in this formulation (GN and AN) are precisely those which are realised by the smallest number of VSO languages in question, the three languages of type 3. By contrast, the languages whose properties are explicitly referred to in formulation

a) (NA and NG) are those which characterise the vast majority of VSO languages, those of type 1. Although both a) and b) are logically equivalent, I shall choose a) for this distributional reason, and shall follow the following general convention which I refer to as:

> The Distributional Convention for Implicational Universals
>
> Where logically equivalent formulations are available for universal implicational statements, choose that formulation whose positive P and Q properties are found in the greatest number of the relevant languages.

The a) formulation is more transparent to read, since the explicit P & Q co-occurrence can be checked directly with far more languages than the P & Q properties in the b) formulation. The -P & Q and -P &-Q values in these statements must always be computed from the P & Q properties which are explicitly mentioned in the implicational universal, and hence in the event that the positive P & Q properties describe a minority type, vast numbers of languages must be checked against a prior computation of the relevant negative values. We shall also see how this distributional convention makes the distributional generalisation of the next section more transparent.

Consider next the postpositional SOV languages 21 through 24:

21.	SOV/Po/NG/NA	Attested
22.	SOV/Po/NG/AN	No examples
23.	SOV/Po/GN/AN	Attested
24.	SOV/Po/GN/NA	Attested

Again, three noun-modifier types are attested, and one unattested, the by now familiar typological pattern defined by a unilateral implication. The following implicational universal defines the three attested noun-modifier types into existence and rules out the fourth:

c) If the order of noun and adjective is AN (P), then the order of genitive and noun is GN (Q), i.e. AN → GN

i/	P & Q	AN & GN	(23)
ii/	-P & Q	NA & GN	(24)
iii/	-P &-Q	NA & NG	(21)
* iv/	P &-Q	AN & NG	(22 - no examples)

This formulation is to be preferred to the logically equivalent d) on account of the distributional convention. The properties AN & GN are found in the majority type, 23.

d) If the order of noun and genitive is NG (P), then the order of adjective and noun is NA (Q), i.e. NG → NA

Notice now that the statements a) NA → NG and c) AN → GN are mirror-images of one another. They are both compatible with the co-occurrences NA & NG and AN & GN, in which both modifiers are on the same side relative to the noun, but only a) allows for AN & NG, and only b) allows for NA & GN, i.e. they are incompatible with regard to the mixed co-occurrences in which adjective and genitive occur on opposite sides from one another.

Statements a) and c) account for all the noun-modifier co-occurrences in the prepositional VSO and postpositional SOV languages. We must now add to them information which makes the respective correlations with VSO & Prep and SOV & Postp explicit. Recall first that neither prepositional nor postpositional VSO languages allow the offending co-occurrences NA & GN:

4. VSO/Pr/GN/NA No examples
8. VSO/Po/GN/NA No examples

The absence of NA & GN is characteristic of all VSO languages, and we can therefore insert just VSO alone as an implicational antecedent into a), to create our first complete universal implication, statement A:

A If a language has VSO (P), then if the order of noun and adjective is NA (R), then the order of noun and genitive is NG (S), i.e.
VSO → (NA → NG)

Recall also that neither prepositional nor postpositional SOV languages allow the offending co-occurrence AN & NG:

18. SOV/Pr/NG/AN No examples
22. SOV/Po/NG/AN No examples

Again we can correlate the possible and impossible noun-modifier co-occurrences with SOV alone, to make our second complete universal implication, statement B:

B If a language has SOV (P), then if the order of noun and adjective is AN (R), then the order of genitive and noun is GN (S), i.e.
SOV → (AN → GN)

Both statements A and B are of the logical form P → Q, where Q consists in turn of an implication R → S. Let us tabulate the possible and impossible co-occurrence types which are defined by these two statements. The following is in effect a truth table for the implication VSO → (NA → NG), in which the possible or valid co-occurrences are unstarred, and the impossible or invalid co-occurrences starred:

A VSO → (NA → NG) i.e. $P \rightarrow_Q (R \rightarrow S)_Q$

i/	P & Q	a) P & R & S	VSO & NA & NG	(1)
		b) P &-R & S	VSO & AN & NG	(2)
		c) P &-R &-S	VSO & AN & GN	(3, 7)
ii/	-P & Q	a)-P & R & S	-VSO & NA & NG	
		b)-P &-R & S	-VSO & AN & NG	
		c)-P &-R &-S	-VSO & AN & GN	
iii/	-P &-Q	a)-P & R &-S	-VSO & NA & GN	
* iv/	P &-Q	a) P & R &-S	VSO & NA & GN	(4, 8 - no examples)

We shall return to the co-occurrence types with -VSO presently. Notice for the moment that implication A makes exceptionless predictions about VSO languages.

Consider now the corresponding truth table for universal implication B:

B SOV → (AN → GN) i.e. $P \rightarrow_Q (R \rightarrow S)_Q$

i/	P & Q	a) P & R & S	SOV & AN & GN	(19, 23)
		b) P &-R & S	SOV & NA & GN	(24)
		c) P &-R &-S	SOV & NA & NG	(17, 21)
ii/	-P & Q	a)-P & R & S	-SOV & AN & GN	
		b)-P &-R & S	-SOV & NA & GN	
		c)-P &-R &-S	-SOV & NA & NG	
iii/	-P &-Q	a)-P & R &-S	-SOV & AN & NG	
* iv/	P &-Q	a) P & R &-S	SOV & AN & NG	(18, 22 - no examp.)

Again this implication makes exceptionless predictions about the noun-modifier co-occurrences in all of Greenberg's SOV languages.

Having discovered the exceptionless validity of these implications defined ultimately on VSO and SOV, notice an interesting bonus. Implications A and B between them actually predict that SVO languages are going to be typologically ambivalent. In order to see why, let us calculate the complete truth values for these implications, converting the negative -VSO and -SOV into their corresponding positive values, as follows:

A VSO → (NA → NG) i.e. $P \rightarrow {}_{Q}(R \rightarrow S)_{Q}$

i/ P & Q	a) P & R & S		VSO & NA & NG	(1)
	b) P &-R & S		VSO & AN & NG	(2)
	c) P &-R &-S		VSO & AN & GN	(3, 7)
ii/ -P & Q	a) -P & R & S	1/	SVO & NA & NG	(9)
		2/	SOV & NA & NG	(17, 21)
	b) -P &-R & S	1/	SVO & AN & NG	(10, 14)
		2/	SOV & AN & NG	(18, 22 - no examp.)
	c) -P &-R &-S	1/	SVO & AN & GN	(11, 15)
		2/	SOV & AN & GN	(19, 23)
iii/ -P &-Q	a) -P & R &-S	1/	SVO & NA & GN	(12, 16)
		2/	SOV & NA & GN	(24)
* iv/ P &-Q	a) P & R &-S		VSO & NA & GN	(4, 8 - no examp.)

B SOV → (AN → GN) i.e. $P \rightarrow {}_{Q}(R \rightarrow S)_{Q}$

i/ P & Q	a) P & R & S		SOV & AN & GN	(19, 23)
	b) P &-R & S		SOV & NA & GN	(24)
	c) P &-R &-S		SOV & NA & NG	(17, 21)
ii/ -P & Q	a) -P & R & S	1/	SVO & AN & GN	(11, 15)
		2/	VSO & AN & GN	(3, 7)
	b) -P &-R & S	1/	SVO & NA & GN	(12, 16)
		2/	VSO & NA & GN	(4, 8 - no examp.)
	c) -P &-R &-S	1/	SVO & NA & NG	(9)
		2/	VSO & NA & NG	(1)
iii/ -P &-Q	a) -P & R &-S	1/	SVO & AN & NG	(10, 14)
		2/	VSO & AN & NG	(2)
iv/ P &-Q	a) P & R &-S		SOV & AN & NG	(18, 22 - no examp.)

Since universal A permits -P to co-occur both with Q (ii/) and with -Q (iii/), and since -P represents the two non-VSO types, SVO and SOV, this universal is allowing SVO and SOV languages to co-occur with all noun-modifier co-occurrences which make Q either true or false. Hence, both SVO and SOV (-P) are allowed to co-occur with the three valid (Q) co-occurrences defined by NA → NG, and with the invalid (-Q) co-occurrence NA & GN. In effect, no restrictions are placed on the

noun-modifier orders co-occurring with non-VSO languages. For SVO languages these predictions are completely fulfilled. SVO co-occurs both with the three noun-modifier co-occurrences of VSO languages (NA & NG, AN & NG, AN & GN), and with the fourth noun-modifier co-occurrence which is excluded in VSO languages (NA & GN). For SOV languages all but one of the predicted co-occurrences are attested (ii/b)2/). But notice that this is precisely the co-occurrence (18 and 22) which is ruled out by universal B: SOV & AN & NG. All the co-occurrences permitted by universal A which are not ruled out by universal B are attested in the data sample, and the one co-occurrence which is ruled out by A is totally unattested. Universal A therefore makes exactly the right predictions. A generalisation which is independently justified for VSO languages predicts automatically that SVO languages are typologically ambivalent. It also makes, in conjunction with universal B, the correct predictions for SOV languages. However, given that one universal allows for the existence of SOV & AN & NG languages, and that the other does not, we need to introduce the following interpretation convention:

> The Interpretation Convention
>
> A universal word order co-occurrence which is excluded by any one universal implication shall be considered an impossible language type, even if this same co-occurrence is logically permitted as part of the total predictions of some other universal.

This convention seems reasonable enough, and it is in fact implicit in Greenberg's work. For example, his universal (3) (VSO → Prep) allows for both SVO and SOV languages to have prepositions as well (-P & Q). But the possibility of SOV co-occurring with prepositions is then negated by universal (4) (SOV → Postp).

Universal B allows for non-SOV languages to have both SOV-type noun-modifier co-occurrences (-P & Q), and non-SOV type noun-modifier co-occurrence (-P &-Q, i.e. AN & NG). SVO languages have all possibilities, and VSO languages have all but the unattested co-occurrence ii/b)2/, which is explicitly excluded by universal A, and hence predicted to be non-existent by the interpretation convention. Again, universal B makes exactly the right predictions. All the language types which are predicted to be possible do occur, and the impossible type P &-Q is totally unattested. These results should encourage us to search for exceptionless generalisations in universal word order studies. To set up mere co-occurrence tendencies, given facts like these, is to fail to capture generalisations in a very serious way.

There are a total of eight unattested cells in Appendix II. Universals A and B rule out four of them in a principled way (4, 8, 18, 22). We must now attempt to account for the others. Consider VSO languages again.

Notice that as far as types 1 through 4 are concerned

1. VSO/Pr/NG/NA Attested
2. VSO/Pr/NG/AN Attested
3. VSO/Pr/GN/AN Attested
4. VSO/Pr/GN/NA No examples

we could have set up a universal corresponding to universal A with Prep instead of VSO as implicational antecedent, in order to account for the same range of noun-modifier co-occurrences:

e) If a language has prepositions (P), then if the order of adjective and noun is NA (R), then the order of genitive and noun is NG (S), i.e. Prep $\rightarrow$ (NA $\rightarrow$ NG)

Similarly, for the SOV languages 21 through 24

21. SOV/Po/NG/NA Attested
22. SOV/Po/NG/AN No examples
23. SOV/Po/GN/AN Attested
24. SOV/Po/GN/NA Attested

we could have accounted for the same range of noun-modifier co-occurrences with a version of B in which Postp replaced SOV:

f) If a language has postpositions (P), then if the order of noun and adjective is AN (R), then the order of genitive and noun is GN (S), i.e. Postp $\rightarrow$ (AN $\rightarrow$ GN)

Universal e) defines the following possible and impossible co-occurrence types. For the moment we shall limit the exemplifications to prepositional VSO and postpositional SOV languages only.

e) Prep $\rightarrow$ (NA $\rightarrow$ NG) i.e. $P \rightarrow_Q (R \rightarrow S)_Q$

i/	P & Q	a) P & R & S	Prep & NA & NG	(1)
		b) P &-R & S	Prep & AN & NG	(2)
		c) P &-R &-S	Prep & AN & GN	(3)
ii/	-P & Q	a)-P & R & S	Postp & NA & NG	(21)
		b)-P &-R & S	Postp & AN & NG	(22 - no examp.)
		c)-P &-R &-S	Postp & AN & GN	(23)
iii/	-P &-Q	a)-P & R &-S	Postp & NA & GN	(24)
* iv/	P &-Q	a) P & R &-S	Prep & NA & GN	(4 - no examp.)

And universal f) defines the following possibilities and impossibilities:

f) Postp $\rightarrow$ (AN $\rightarrow$ GN) i.e. P $\rightarrow_{Q}$ (R $\rightarrow$ S)$_{Q}$

i/	P & Q	a) P & R & S	Postp & AN & GN	(23)
		b) P &-R & S	Postp & NA & GN	(24)
		c) P &-R &-S	Postp & NA & NG	(20)
ii/	-P & Q	a)-P & R & S	Prep & AN & GN	(3)
		b)-P &-R & S	Prep & NA & GN	(4 - no examp.)
		c)-P &-R &-S	Prep & NA & NG	(1)
iii/	-P &-Q	a)-P & R &-S	Prep & AN & NG	(2)
* iv/	P &-Q	a) P & R &-S	Postp & AN & NG	(22 - no examp.)

Universals e) and f) in conjunction with one another make exactly the right predictions for the eight language types, 1 through 4 and 21 through 24. The unattested -P & Q co-occurrence according to universal e) (22: Postp & AN & NG) is precisely the one which universal f) rules out, while the unattested -P & Q co-occurrence with universal f) (4: Prep & NA & GN) is exactly the one which universal e) rules out. All the co-occurrence types which are permitted to be possible do occur, while the impossible ones are totally unattested.

But let us expand the data base, and notice an intriguing generalisation. Consider all SOV languages:

21.	SOV/Po/NG/NA	Attested	17.	SOV/Pr/NG/NA	Attested
22.	SOV/Po/NG/AN	No examples	18.	SOV/Pr/NG/AN	No examples
23.	SOV/Po/GN/AN	Attested	19.	SOV/Pr/GN/AN	Attested
24.	SOV/Po/GN/NA	Attested	20.	SOV/Pr/GN/NA	No examples

Universal B accounts for the impossibility of 18 and 22. But there is one other unattested SOV type: 20. When a language has both SOV and prepositions it is evidently impossible to have a co-occurring GN & NA. But why? It transpires that Prep & GN & NA is precisely the co-occurrence which is ruled out by implication e) Prep $\rightarrow$ (NA $\rightarrow$ NG). Hence, although GN & NA is perfectly compatible with SOV (cf i/b) in the truth table for universal B), its co-occurrence with Prep is ruled out by universal e) (cf iv/ in the table under e)). There is therefore no strict constraint against SOV co-occurring with prepositions, even though this is a minority co-occurrence. But when SOV & Prep do co-occur the possible noun-modifier co-occurrences are exactly those which can be shown to co-occur with SOV and Prep independently. The two SOV & Prep types (17 and 19) are those in the intersection of the two sets of noun-modifier co-occurrence possibilities defined by universal B and universal e):

Universal B	Universal e)	Co-occurrence types
SOV & AN & GN	Prep & AN & GN	SOV/Pr/AN/GN (19) Attested
SOV & NA & GN	* Prep & NA & GN	* SOV/Pr/NA/GN (20) No examp.
SOV & NA & NG	Prep & NA & NG	SOV/Pr/NA/NG (17) Attested
* SOV & AN & NG	Prep & AN & NG	* SOV/Pr/AN/NG (18) No examp.

Such facts provide further support for universals B and e). They also support our interpretation convention: a co-occurrence which is ruled out by any one universal is an impossible language type, even if it is compatible with another universal. Notice also that the one postpositional VSO language in Greenberg's sample (type 7) is also within the intersection of the two sets of noun-modifier co-occurrence possibilities defined by universals A and f):

Universal A	Universal f)	Co-occurrence types
VSO & NA & NG	Postp & NA & NG	VSO/Po/NA/NG (5) No examp.
VSO & AN & NG	* Postp & AN & NG	* VSO/Po/AN/NG (6) No examp.
VSO & AN & GN	Postp & AN & GN	VSO/Po/AN/GN (7) Attested
* VSO & NA & GN	Postp & NA & GN	* VSO/Po/NA/GN (8) No examp.

There are therefore only two potential noun-modifier co-occurrences which could be attested in prepositional SOV and in postpositional VSO languages. I regard it as highly significant that the two attested prepositional SOV types are exactly the two predicted, and that the one postpositional VSO language falls within one of the two predicted possibilities. However, these generalisations suggest that we should also be able to find postpositional VSO languages of type 5 (VSO/Po/NA/NG) outside this data sample. But within the present data sample we are going to have to exclude this type altogether by another universal (universal F below) on account of its total absence, even though the word order patterns derivable from SOV and other VSO languages suggest that it should be a possibility.

Universals e) and f), when interpreted in conjunction with universals A and B (and also F), therefore account for all VSO and SOV languages in Greenberg's Appendix, without exceptions. Consider now SVO languages. We have seen that SVO may co-occur with the noun-modifier combination NA & GN, which is totally impossible in VSO languages, and with AN & NG, which is totally impossible in SOV languages. Universals A and B predict this ambivalence. Notice now that it is precisely in SVO languages that universals e) and f) are no longer fully operative. Thus, the P &-Q co-occurrence according to universal e) (Prep & NA & GN) finds an exception, albeit in the form of just one language, in SVO type 12. And the P &-Q co-occurrence according to universal f) (postp & AN & NG) finds exceptions in SVO type 14. Hence, there are no exceptions

to e) and f) as they stand, within VSO and SOV languages, but only in languages with SVO.

This further ambivalence of SVO can again be predicted in a principled way on the basis of regularities which can be established independently. For the data upon which we erected universals e) and f) included only VSO and SOV languages. We have seen that prepositions may occur both in VSO and in SOV languages, and that postpositions are attested both in SOV and VSO. Hence, we are going to have to allow for both adposition orders to co-occur with both these verb positions. Since SVO languages do not produce exceptionless co-occurrences for universals e) and f), we are going to have to relativise these two universals to VSO and SOV languages, while allowing for the co-occurrence of both adposition orders with VSO and SOV. This can be done by adapting universal e) to create our third complete universal, implication C:

C If a language has both prepositions and either VSO or SOV (P), then if the order of noun and adjective is NA (R), then the order of genitive and noun is NG (S), i.e.
Prep & (VSO v SOV) $\rightarrow$ (NA $\rightarrow$ NG)

and by similarly adapting universal f) to create our fourth complete universal, implication D:

D If a language has both postpositions and either VSO or SOV (P), then if the order of noun and adjective is AN (R), then the order of genitive and noun is GN (S), i.e.
Postp & (VSO v SOV) $\rightarrow$ (AN $\rightarrow$ GN)

As we shall see, we still require universals A and B in conjunction with C and D.

Let us explore the exact co-occurrences which are defined by C and D:

C Prep & (VSO v SOV) $\rightarrow$ (NA $\rightarrow$ NG), i.e. $P \rightarrow {}_{Q}(R \rightarrow S)_{Q}$

i/ P & Q	a) P & R & S	1/	Prep & VSO & NA & NG (1)
		2/	Prep & SOV & NA & NG (17)
	b) P &-R & S	1/	Prep & VSO & AN & NG (2)
		2/	Prep & SOV & AN & NG (18-no examp)
	c) P &-R &-S	1/	Prep & VSO & AN & GN (3)
		2/	Prep & SOV & AN & GN (19)
ii/-P & Q	a)-P & R & S	1/	Postp & VSO & NA & NG (5-no examp)
		2/	Postp & SOV & NA & NG (21)
		3/	Prep & SVO & NA & NG (9)
		4/	Postp & SVO & NA & NG (13-no examp)

b)-P &-R & S
1/ Postp & VSO & AN & NG (6-no examp)
2/ Postp & SOV & AN & NG (22-no examp)
3/ Prep & SVO & AN & NG (10)
4/ Postp & SVO & AN & NG (14)

c)-P &-R &-S
1/ Postp & VSO & AN & GN (7)
2/ Postp & SOV & AN & GN (23)
3/ Prep & SVO & AN & GN (11)
4/ Postp & SVO & AN & GN (15)

iii/-P &-Q a)-P & R &-S
1/ Postp & VSO & NA & NG (8-no examp)
2/ Postp & SOV & NA & GN (24)
3/ Prep & SVO & NA & GN (12)
4/ Postp & SVO & NA & GN (16)

• iv/ P &-Q a) P & R &-S
1/ Prep & VSO & NA & GN (4-no examp)
2/ Prep & SOV & NA & GN (20-no examp)

D Postp & (VSO v SOV) $\rightarrow$ (AN $\rightarrow$ GN), i.e. P $\rightarrow_Q (R \rightarrow S)_Q$

i/ P & Q a) P & R & S
1/ Postp & VSO & AN & GN (7)
2/ Postp & SOV & AN & GN (23)

b) P &-R & S
1/ Postp & VSO & NA & GN (8-no examp)
2/ Postp & SOV & NA & GN (24)

c) P &-R &-S
1/ Postp & VSO & NA & NG (5-no examp)
2/ Postp & SOV & NA & NG (21)

ii/-P & Q a)-P & R & S
1/ Prep & VSO & AN & GN (3)
2/ Prep & SOV & AN & GN (19)
3/ Postp & SVO & AN & GN (15)
4/ Prep & SVO & AN & GN (11)

b)-P &-R & S
1/ Prep & VSO & NA & GN (4-no examp)
2/ Prep & SOV & NA & GN (20-no examp)
3/ Postp & SVO & NA & GN (16)
4/ Prep & SVO & NA & GN (12)

c)-P &-R &-S
1/ Prep & VSO & NA & NG (1)
2/ Prep & SOV & NA & NG (17)
3/ Postp & SVO & NA & NG (13-no examp)
4/ Prep & SVO & NA & NG (9)

iii/-P &-Q a)-P & R &-S
1/ Prep & VSO & AN & NG (2)
2/ Prep & SOV & AN & NG (18-no examp)
3/ Postp & SVO & AN & NG (14)
4/ Prep & SVO & AN & NG (10)

• iv/ P &-Q a) P & R &-S
1/ Postp & VSO & AN & NG (6-no examp)
2/ Postp & SVO & AN & NG (22-no examp)

For clarity of exposition I have kept property P unanalysed in these formulae. A more accurate representation of the logical structure of universals C and D would be:

$$_{P}(T \;\&\; (W \vee Y))_{P} \rightarrow {}_{Q}(R \rightarrow S)_{Q}$$

In accordance with the assignments of truth values to & and v, there will be two valid realisations of the positive term P:

1/ T & W (e.g. Prep & VSO in implication C)
2/ T & Y (e.g. Prep & SOV in implication C)

and four possible realisations of -P:

1/-T & W (e.g. Postp & VSO in implication C)
2/-T & Y (e.g. Postp & SOV ")
3/ T &-(W v Y) (e.g. Prep & SVO in implication C)
4/-T &-(W v Y) (e.g. Postp & SVO ")

Despite their somewhat complex appearance universals C and D are quite straightforward. All that C is saying is that universal e) Prep → (NA → NG) holds only in VSO and SOV languages, and D merely says that f) Postp → (AN → GN) also holds only in VSO and SOV languages. The whole purpose of C is to exclude the co-occurrence Prep & NA & GN in VSO and SOV languages (4 and 20), while allowing it in SVO languages (12). And the purpose of D is to exclude the co-occurrence of Postp & AN & NG in VSO and SOV languages (6 and 22), while allowing it in SVO languages (14). The unattested co-occurrences which C and D permit are all excluded by other universals. In the table for C there are six such logically possible but unattested types: 18, 5, 13, 6, 22, 8. Of these universal D excludes 6 and 22, universal A excludes 8, universal B excludes 18 (and also 22), and universal F will exclude 5 and 13. In the table for D there are also six logically possible unattested types: 8, 5, 4, 20, 13, 18. Of these universal C excludes 4 and 20, universal A excludes 8 (and also 4), universal B excludes 18, while universal F excludes 5 and 13.

Universals C and D, in conjunction with the other universals, generate exceptionless predictions for all 24 co-occurrence types in Appendix II. However, there is one small anomaly which must be pointed out. If it were not for the SVO co-occurrence types 12 (SVO/Pr/GN/NA) and 14 (SVO/Po/NG/AN), which are attested, it would have been possible to propose e) and f), rather than the more complicated and less general C and D, as our third and fourth implicational universals. And yet the size of these two types is unexpectedly small. Type 12 has just one language (Arapesh) and type 14 just a small number (Rutulian and other Daghestan languages). Should we therefore abandon the much more powerful generalisations e) and f) on account of just two named languages,

or should we not propose, in this single instance, that these two named languages are simply exceptions? In fact these two languages are going to have to be exceptions either to the implicational generalisations or to the distributional generalisation of the next section. If therefore we were to regard them as exceptions, their exceptional status would explain their distributional rarity. We shall also discuss some general theoretical reasons for preferring this latter alternative in section (F). However, given Appendix II as it stands, implications C and D, and not e) and f), are the correct statements distinguishing attested from non-attested co-occurrences.

Universals A, B, C and D between them correctly rule out six of the eight unattested types in Appendix II: 4, 8, 18, 22, 20, 6. We still have to account for 5 and 13. Consider first 13:

13. SVO/Po/NG/NA No examples

This is the only unattested SVO type in the corpus. To what could its absence be attributed? There is in fact a remarkable pattern here. Type 13 has postpositions co-occurring with two noun-modifier orders, in both of which the noun is leftmost and hence initial within the NP. Consider the converse state of affairs, SVO languages in which there are prepositions and in which the noun is rightmost or final within the NP:

11. SVO/Pr/GN/AN Norwegian, Swedish, Danish

It transpires that these three languages all have one productive noun-modifier occurring after the noun: relative clauses (in addition to a morphologically-bound postposed definite article). Hence, the noun is not entirely final within the NP. If we therefore supplement 11 with the property relative clause + noun (RelN) we get an unattested type 11':

11'. SVO/Pr/GN/AN/RelN No examples

Comparison of 13 with 11' therefore reveals a clear generalisation: in SVO languages, if the adposition element is initial relative to its modifiers or operators (i.e. if the language is prepositional), then the noun cannot occur in absolute final position relative to its operators and modifiers (11'); and if the adposition element is final within the adposition phrase (i.e. if the language is postpositional), then the noun cannot occur in absolute initial position relative to its modifiers within the NP (13). Evidently, therefore, the absolute initial or final position of the noun within the NP is sensitive to the structure of the adposition phrase within SVO languages.

When we look outside SVO languages we can in fact state this constraint in a slightly more general way. Thus, although Po/NG/NA does not occur in SVO, it does not occur in VSO either, though it does in SOV:

5. VSO/Po/NG/NA No examples
13. SVO/Po/NG/NA No examples
21. SOV/Po/NG/NA Attested

Similarly, Pr/GN/AN/RelN does not occur in either VSO or SVO (cf Antinucci (1976)), though it does occur in the one pertinent SOV language (Amharic):

3'. VSO/Pr/GN/AN/RelN No examples
11'. SVO/Pr/GN/AN/RelN No examples
19'. SOV/Pr/GN/AN/RelN Attested

There are numerous formulations that we could adopt in order to account for these facts. Without further discussion I shall simply propose the following. First, universal E:

E If a language has prepositions and either SVO or VSO order (P), then it does not have noun-final order within the NP (Q), i.e. Prep & (SVO v VSO) → -N-final

This universal rules out 11' and 3', while allowing for 19'. I shall not exemplify the full range of co-occurrence possibilities. It should be readily transparent that the logically permitted types do exist, while the non-permitted types do not.

Secondly, universal F:

F If a language has postpositions and either SVO or VSO order (P), then it does not have noun-initial order within the NP (Q), i.e. Postp & (SVO v VSO) → -N-initial

This universal allows for 21 and rules out both 5 and 13. Again, the exemplifications for the different values of P and Q should be transparent and I shall not go into them. Notice only that the exclusion of type 5 along with 13 has been brought about out of convenience. We have already presented evidence suggesting that 5 (VSO/Po/NG/NA) could potentially occur, and in the next section I shall suggest that there is a distributional reason only why it does not.

We have presented a total of six exceptionless implications in this section covering the data of Greenberg's Appendix II. Between them they exclude (in conjunction with the interpretation convention) all eight unattested types while allowing for the sixteen that are attested. They can be summarised as follows (with the excluded co-occurrence types in parentheses):

A VSO → (NA → NG) (4, 8)
B SOV → (AN → GN) (18, 22)

C Prep & (VSO v SOV) → (NA → NG) (4, 20)

D Postp & (VSO v SOV) → (AN → GN) (6, 22)

E Prep & (SVO v VSO) → -N-final (11', 3')

F Postp & (SVO v VSO) → -N-initial (5, 13)

In addition, we have suggested that the simpler and more powerful implications e) and f) might conceivably replace C and D, since they have so few exceptions:

e) Prep → (NA → NG) (4, 12, 20)

f) Postp → (AN → GN) (6, 14, 22)

e) and f) are therefore the only statistical universals discussed in this section. If we adopt C and D then 12 and 14 are predicted to exist, but they become exceptions to the distributional generalisation. If we adopt e) and f), then 12 and 14 are wrongly predicted not to occur, but they are then no longer exceptions to the distributional generalisation, since languages which are exceptions to statistical statements will be predicted to have a very low frequency of occurrence.

Section (E): The Distributional Generalisation

Now that we have formulated the implicational regularities which give structure to the balance between attested and non-attested co-occurrence types in Appendix II, a distributional generalisation covering the attested types becomes readily transparent.

Consider first prepositional VSO languages. Implication A VSO → (NA → NG) allows for three noun-modifier co-occurrences in VSO languages. Here are the distributions across the three:

1. VSO/Pr/NG/NA 15 named lgs + Celtic lgs and various Polynesian, Austronesian and Ote-Mangue lgs
2. VSO/Pr/NG/AN 4 named lgs + various Philippine Austronesian lgs
3. VSO/Pr/GN/AN 3 named lgs

Evidently, as we follow the implication NA → NG down through its permitted co-occurrences P & Q (NA & NG), -P & Q (AN & NG), and -P &-Q (AN & GN) there is a decline in the quantity of languages in each cell. The P & Q languages (type 1) have by far the most, next comes the -P & Q languages (type 2), and finally the -P &-Q languages (type 3) are the least frequent of all. The decline is not completely steady, however. There is a big difference between types 1 and 2, and

only a small difference between types 2 and 3. We shall see that there is a reason for this. For the moment notice only the gross relative decline in quantity down the three types.

Notice secondly that exactly the same pattern occurs in postpositional SOV languages. Implication B SOV → (AN → GN) again allows for three noun-modifier co-occurrences in SOV languages, for which the distributions are:

23. SOV/Po/GN/AN 24 named lgs + various Aryan lgs of India, and various Finno-Ugric, Caucasian and Sino-Tibetan lgs
24. SOV/Po/GN/NA 23 named lgs + most Australian lgs
21. SOV/Po/NG/NA 6 named lgs + various SE Australian lgs

Again, as we follow the implication AN → GN down the three possible co-occurrences of P & Q (AN & GN), -P & Q (NA & GN) and -P & -Q (NA & NG) there is a decline in distribution. Type 23 has one more named language than type 24, and mentions four groups of languages rather than one, while type 21 mentions only six named languages and one group. What the exact quantities are of the languages subsumed under the various groups in Greenberg's sample is not something that Greenberg tells us. However, the quantitative difference between 24 and 21 and between 23 and 21 is clear enough, and the areal and genetic coverage of type 23 is much broader than type 24.

Notice a third set of facts. The three prepositional SVO languages which are regular according to implication e) Prep → (NA → NG) have a distribution which is very similar to prepositional VSO languages:

9. SVO/Pr/NG/NA 14 named lgs + Romance, West Atlantic and Bantu lgs, and various Benue-Congo, Chad, Thai and Austronesian lgs
10. SVO/Pr/NG/AN 8 named lgs
11. SVO/Pr/GN/AN 3 named lgs

This decline in distribution through NG/NA (9), NG/AN (10), and GN/AN (11) parallels the decline in 1 through 3. In addition the difference between type 9 and type 11 is almost exactly like that between 1 and 3, although the intermediate -P & Q type, 10, has twice as many named languages as the corresponding intermediate type 2, in addition to a wider genetic and areal coverage.

We have now seen a total of nine of the sixteen attested and permitted co-occurrence types. For prepositional VSO and SVO languages there is a decline in frequency which follows the arrow:

VSO/SVO (pr)	NA & NG	P & Q	↓ decrease
	AN & NG	-P & Q	
	AN & GN	-P &-Q	

The number of languages evidently decreases as the noun becomes progressively final within the NP and stands to the right of its modifiers. Now if we regard all the arguments of the verb, i.e. direct object, indirect object, prepositional phrase, and the subject, as modifiers of the verb, just like adjective and genitive etc. are modifiers of the noun, then VSO languages are languages in which the verb stands to the extreme left of its modifiers, while in SVO languages the verb stands to the left of all modifiers but for the subject. It is clear that by far the most frequent VSO and SVO languages are those in which the noun occurs on the same side relative to its modifiers (leftmost: NA & NG) as the verb does relative to its modifiers (again leftmost). And as the noun moves away from the leftmost position and is gradually preceded by its modifiers, so there is a gradual decline in the number of attested languages. The generalisation which begins to emerge is, therefore, that frequency of attested co-occurrence type correlates in a principled way with the balance between the number of preposed and postposed modifiers across the different modifier categories. The more similar and harmonic this balance is between preposing and postposing, the greater is the number of attested languages.

Consider again the distributional decline for SOV languages:

SOV (Po)	AN & GN	P & Q	↓ decrease
	NA & GN	-P & Q	
	NA & NG	-P &-Q	

Greenberg's SOV languages are either rigid or non-rigid. In the former case the verb occurs in absolute final position within the clause, in the latter case adverbials and prepositional phrases (and much more rarely, indirect objects) may stand after the verb. In either event the verb is predominantly rightmost within its clause, and the most frequent noun-modifier type, AN & GN, also has the noun rightmost within the NP following its modifiers. The decline in distribution then correlates with the increasing leftward movement of the noun. As the preposing and postposing balance becomes more disharmonic between the verb and its modifiers, and the noun and its modifiers, so the number of languages decreases.

Notice two further predictions which our emerging distributional generalisation makes about VSO and SOV languages. If this generalisation is correct, we would expect that the rigidity or otherwise of verb-final order will be reflected in the preposing/postposing balance within the

NP, and that the difference between VSO and SVO should also be matched by noun-modifier orders. Greenberg's detailed 30-language sample enables us to test these hypotheses.

Consider first SOV. To the extent that the verb occurs rigidly finally, we would expect the noun to occur rigidly finally within the NP. And to the extent that the verb can regularly precede some of its modifiers, we would expect the noun to precede some of its modifiers. Thus, we predict that most rigid SOV languages will have AN & GN since they are most harmonic with the rightmost position of the noun. But non-rigid SOV languages are most harmonic with noun-modifier co-occurrences in which at least some modifiers follow the noun, just as some verb-modifiers follow the verb. There are 11 SOV languages in Greenberg's 30-language sample, of which six are rigid and five non-rigid. It transpires that all six rigid SOV languages do indeed have AN & GN (Burushaski, Hindi, Kannada, Japanese, Turkish, and also Burmese which actually has both AN/NA & GN). By contrast, four of the five non-rigid SOV languages have NA & GN (Basque, Chibcha, Loritja and Nubian) while the fifth, Quechua, has at least one productive postnominal modifier: NRel (in addition to RelN) and AN & GN. The SOV languages with noun-modifier postposing are therefore the non-rigid ones, while rigid verb-finality matches noun-finality.

With VSO and SVO we expect the more leftmost position of the verb in VSO to be matched by a more leftmost position of the noun than in SVO languages. There are 6 VSO languages and 10 (prepositional) SVO languages in Greenberg's 30-language sample. It transpires that all six VSO languages have NA & NG, just as predicted (Berber, Hebrew, Maori, Masai, Welsh and Zapotec). Of the ten SVO languages, there are six with NA & NG (Fulani, Italian, Malay, Swahili, Thai, Yoruba) and four which have at least one preposed noun-modifier (Greek, Maya, Norwegian, Serbian). Thus, whereas 100% of the VSO languages have NA & NG, only 60% of the SVO languages have this co-occurrence. This is roughly the statistical difference one would expect to correlate with the preposing of a single noun-modifier before the verb in SVO languages. It also ties in with our observation above that there were more SVO languages than VSO languages in the Appendix with the co-occurrence Pr/NG/AN (cf 10 versus 2).

We can now state our distributional generalisation. I shall refer to it as the 'principle of cross-category harmony' (CCH):

The Principle of Cross-Category Harmony (CCH)

The more similar the quantitative balance between the pre- and postposing of modifiers across the different modifier categories (N, V etc.), the greater is the number of such languages.

The nine Appendix II co-occurrences considered so far point to the existence and validity of this principle, as do 27 of the 30 languages in Greenberg's detailed sample (ie. all but three postpositional SVO languages, a type we have yet to consider).

Notice that this principle of cross-category harmony works equally for languages which obey the different implicational generalisations distinguishing possible from impossible language types, e.g. e) versus f), A versus B. In other words, its predictions are quite separate from these logical implications. It stipulates that within any set of co-occurrences permitted by some logical implication, the distribution of languages will follow CCH, the more frequent languages being those with the most harmonic preposing and postposing balance across the categories.

According to Vennemann, adposition phrases can also be regarded as instances of operator-operand or modifier-modified constructions, the adposition element itself being the modified constituent. When the modifiers follow, the language is prepositional, when they precede, the language is postpositional. If this is correct, then CCH makes predictions about language frequencies involving adposition order. Prepositional languages will be most harmonic with verb-initial position, and progressively less harmonic as the verb moves towards final position, whereas postpositional languages will exhibit exactly the opposite tendency. The following figures bear this prediction out. I have calculated the percentages of VSO, SVO and SOV languages which are prepositional and postpositional respectively. Since the numbers of languages in Greenberg's groups and families are unknown, I have limited the calculations to the named languages only. For example, 22 out of 23 named VSO languages (95.7%) are prepositional, etc.

Prep & VSO	$^{22}/_{23}$	=	95.7%	
SVO	$^{26}/_{41}$	=	63.4%	decrease
SOV	$^{5}/_{58}$	=	8.6%	
Postp & SOV	$^{53}/_{58}$	=	91.4%	
SVO	$^{15}/_{41}$	=	36.6%	decrease
VSO	$^{1}/_{23}$	=	4.3%	

Thus, the decrease in prepositional languages correlates exactly with decreasing harmony with verb position, as does the postpositional decrease. Furthermore, the difference between the middle figures

(Prep & SVO = 63.4% and Postp & SVO = 36.6%) seems to be a natural consequence of the fact that prepositions are actually more harmonic with SVO order than are postpositions. In SVO languages the verb still occurs to the left of most of its modifiers, the subject being the only preceding modifier, and in prepositional languages the preposition generally occurs to the left of all of its modifiers. But postpositions generally occur to the right of all their modifiers.

CCH also predicts that prepositions will be most harmonic with noun-initial (NA/NG) co-occurrences, and postpositions most harmonic with noun-final (AN/GN) co-occurrences. As for the noun-medial orders (AN/NG and NA/GN) each adposition order is only logically possible with one of these two. We can factor out these different logical possibilities by setting up the distributional types noun-initial, noun-medial and noun-final, and by correlating adposition order with them. By adding the total number of noun-initial, noun-medial and noun-final named languages in the Appendix we can calculate the percentages of each type which have prepositions and postpositions, as follows:

Prep & N-initial	33/39	=	84.6%	
N-medial	13/46	=	28.3%	decrease
N-final	8/37	=	21.6%	
Postp & N-final	29/37	=	78.4%	
N-medial	33/46	=	71.7%	decrease
N-initial	6/39	=	15.4%	

Again, the decrease correctly follows CCH, but the figures for Prep & N-medial (28.3%) are low, while those for Postp & N-medial (71.7%) are high. There is in fact a very good reason for this, although it cannot be seen within the Appendix itself. I show elsewhere (Hawkins (in preparation)) that within prepositional languages the permitted noun-medial co-occurrence (AN/NG by universal e)) entails the co-occurrence of numerous other preposed noun-modifiers (determiners, numerals, possessive adjectives) by implicational laws similar to NA → NG. Hence, an AN/NG prepositional language necessarily has many noun-modifiers preceding the noun, whereas all the adposition modifiers follow the preposition. By contrast, the logically permitted noun-medial co-occurrence in postpositional languages (NA/GN by universal f)) does not actually require any other noun-modifiers to be postposed after the noun, although it does not rule out this possibility. Thus, a postpositional

language like Burmese (SOV) has NA as its unique postposed nominal modifier. Hence, the noun-medial co-occurrence NA & GN is far more harmonic with postpositions, than is AN & NG with prepositions, since NA & GN is indicative of a language state in which the noun may be predominantly final within the NP and the adposition completely final within the adposition phrase. The above figures reflect the implied co-occurrences for the language as a whole therefore.

There is now a third set of correlations which we need to quantify. Noun-initial co-occurrences should be most harmonic with VSO, less harmonic with SVO, and least harmonic with SOV; noun-final co-occurrences should be most harmonic with SOV, less with SVO and least with VSO. Noun-medial co-occurrences are difficult to evaluate because of their different status in prepositional and postpositional languages. However, very roughly we expect noun-medial to be more harmonic with SVO than with either VSO or SOV on account of the verb-peripherality of these latter types. By calculating the percentages of VSO, SVO and SOV languages which have respectively noun-initial, noun-medial and noun-final order we see that all these predictions are in fact fulfilled.

N-initial &	VSO	$^{15}/_{23}$	=	65.2%	
	SVO	$^{14}/_{41}$	=	34.1%	decrease
	SOV	$^{10}/_{58}$	=	17.2%	↓
N-final &	SOV	$^{25}/_{58}$	=	43.1%	
	SVO	$^{8}/_{41}$	=	19.5%	decrease
	VSO	$^{4}/_{23}$	=	17.4%	↓
N-medial &	VSO	$^{4}/_{23}$	=	17.4%	
	SVO	$^{19}/_{41}$	=	46.3%	decrease
	SOV	$^{23}/_{58}$	=	39.7%	↓

The relative differences are predicted by CCH. The absolute figures are also principled. Thus, why should VSO & N-initial (65.2%) be more harmonic than N-final & SOV (43.1%)? The reason is partly that noun-medial NA & GN is compatible with predominantly noun-final order, which makes this co-occurrence harmonic both with rigid SOV and (particularly) with slightly non-rigid SOV, (cf the still rather high N-medial & SOV figure of 39.7%). Partly also it is because SOV

languages come in two varieties, rigid and non-rigid, whereas VSO languages necessarily have the verb leftmost. Hence, the non-rigid SOV languages will not be fully harmonic with N-final, but will prefer N-medial orders instead, whereas VSO will be most harmonic with N-initial and much less harmonic with N-medial (cf the very low N-medial & VSO figure of 17.4%). (NB: the VSO & N-medial figure, 17.4%, is the same as the VSO & N-final figure because we have included only named languages in these figures. The former is actually a slightly larger co-occurrence in the Appendix as a whole).

So far we have considered nine of the sixteen attested Appendix II types in detail, we have considered 27 of the languages in the 30-language sample, and we have just now presented figures for the Appendix as a whole. CCH has so far made exceptionless predictions concerning relative quantities of languages. Of the remaining seven types in Appendix II, two of them are, as we have seen, exceptions to implications e) and f):

12. SVO/Pr/GN/NA 1 named lg
14. SVO/Po/NG/AN 1 named lg

The other five are as follows:

19. SOV/Pr/GN/AN 1 named lg
17. SOV/Pr/NG/NA 4 named lgs
16. SVO/Po/GN/NA 9 named lgs + most Mandingo and Voltaic lgs
15. SVO/Po/GN/AN 5 named lgs
7. VSO/Po/GN/AN 1 named lg

These five non-exceptional language types differ from the other nine in one interesting respect. All the VSO languages in types 1 through 3 are prepositional. Hence, the decrease in frequency here correlates with an increasing noun-modifier disharmony both to the verb position and to the adposition order together, the latter (VSO & Prep) orders being 95.7% harmonic with one another. Similarly, the SOV languages of types 23, 24 and 21 are all postpositional, and so the frequency decrease correlates with noun-modifier postposing progressively at variance both with SOV and with postpositions, these latter being 91.4% harmonic with one another. SVO and Prep in 9 through 11 are also very (63.4%) harmonic with one another, and the noun-modifier preposing correlating with the frequency decrease is again progressively at variance with both. But in the five types above (19, 17 etc.), adposition order and verb position are no longer harmonic with one another. SOV co-occurs with prepositions in 19 and 17, and SVO and VSO with postpositions in 16, 15 and 7. This discrepancy explains why the total number of named languages in these five cells amounts to only 20 out of 122 (or 16.4%), whereas the nine types above house 100 of the 122 named languages i.e. 82%, the remaining 2 languages being the logically exceptional languages of types 12 and 14.

However, I am now going to argue that even the relative numbers of named languages within these five minority types are principled and predictable according to CCH. In other words, I am going to demonstrate that it is no accident that Greenberg discovered exactly this relative balance between his five minority types. In addition, I am going to argue that the unattested type 5

5. VSO/Po/NG/NA

whose existence is predicted to be possible by the independently motivated universals C and D, can be predicted by CCH to be unlikely to occur in a sample of this size.

Consider first types 19 and 17:

19. SOV/Pr/GN/AN 1 named lg
17. SOV/Pr/NG/NA 4 named lgs

In this pair SOV and prepositions make conflicting demands on the noun-modifier preposing/postposing balance. SOV favours noun-finality, prepositions favour the initial position of the noun. SOV, but not prepositional order, is therefore harmonic with GN/AN in 19, whereas prepositional order, but not SOV, is harmonic with NG/NA in 17. Why then does type 17 have four attestations, and type 19 only one? Are we to believe that this is merely an accident?

Remember now that these noun-modifier and verb-modifier orders represent just a small sample of the total number of such modifiers within any one language and that the label SOV is actually ambiguous as between rigid SOV and non-rigid SOV. I have suggested that the actual figures in our correlations between adposition and verb order, adposition and noun order, and noun order and verb order, are a more accurate reflection of the cross-category harmony for these modifier-modified orders within the context of the language as a whole. Where the percentages are high, so is the cross-category harmony represented by the modifier sample, and where the percentages are low, so is the harmony. Where there are significant percentage differences between word order co-occurrences which should be equally harmonic or disharmonic within the Appendix itself, this is therefore taken to reflect cross-category harmony for all the noun and verb-modifiers in the respective languages.

We can now see that the reason why type 17 is more attested than type 19 is quite straightforward. Preposition order is simply that much more harmonic with NG/NA in 17, than is SOV with GN/AN in 19. Thus, even though there are conflicting cross-category harmony requirements in these minority languages, the relatively more attested languages are still going to be those with the greatest cross-category harmony. We can in fact calculate this relative harmony in the following way. Both 19 and 17 have SOV & Prep, so that is not going to decide between them on

distributional grounds. Let us therefore reassess the cross-category harmony between the word orders with respect to which 19 and 17 differ, by deducting the five languages of these two types from the total sample. The cross-category harmony figures which we derive from all the other languages of the Appendix should therefore predict for us which of 19 and 17 is the more harmonic, and hence which is the more frequent. For example, a total of 8 out of 37 N-final languages in the Appendix (21.6%) are prepositional. One of these is the single language of type 19. By excluding it from consideration we are left with a total of 7 out of 36 N-final languages (19.4%) on the basis of which to assess the cross-category harmony between these two word orders. We have to exclude this language in this way, since it is self-defeating to try and explain the presence of a certain number of languages in some co-occurrence cell by including in the calculation the very number of languages that one is trying to predict. In the event that the total number of languages in some cell was very large, for example, yet in a way that was not predictable from the other co-occurrence types of the sample, the inclusion of this large quantity in the calculations might predict a large quantity simply because the calculations included the large quantity itself. However, where the two language types agree (SOV & Prep) we can simply write into the calculations the cross-category harmony figures from the total Appendix (including the languages in question), since this figure will be the same for both types.

Using this method of calculation we can compare the cross-category harmony of 19 and 17 as follows:

19. SOV/Pr/GN/AN	1 named lg	17. SOV/Pr/NG/NA	4 named lgs
i/ SOV & Prep	$^{5}/_{58}$ = 8.6%	i/ SOV & Prep	$^{5}/_{58}$ = 8.6%
ii/ Prep & N-final	$^{7}/_{36}$ = 19.4%	ii/ Prep & N-initial	$^{29}/_{35}$ = 82.9%
iii/ SOV & N-final	$^{24}/_{57}$ = 42.1%	iii/ SOV & N-initial	$^{6}/_{54}$ = 11.1%
	Total = 70,1		Total = 102,6

Thus, the larger quantity of type 17 correlates with higher cross-category harmony totals, as established quite independently of the languages in question.

Compare now types 16 and 15:

16. SVO/Po/GN/NA	9 named lgs	15. SVO/Po/GN/AN	5 named lgs
i/ SVO & Postp	$^{15}/_{41}$ = 36.6%	i/ SVO & Postp	$^{15}/_{41}$ = 36.6%
ii/ Postp & N-medial	$^{24}/_{37}$ = 64.9%	ii/ Postp & N-final	$^{24}/_{32}$ = 75.0%
iii/ SVO & N-medial	$^{10}/_{32}$ = 31.2%	iii/ SVO & N-final	$^{3}/_{36}$ = 8.3%
Total =	132.7	Total =	119.9

The larger quantity of languages again correlates with the higher independently established cross-category harmony figure. Given our formulation of CCH we can not, of course, predict actual numbers of languages in these cells, but only relative size. There is even a correlation between all four sizes and the cross-category harmony figures:

16.	SVO/Po/GN/NA	132.7	9 named lgs
15.	SVO/Po/GN/AN	119.9	5 named lgs
17.	SOV/Pr/NG/NA	102.6	4 named lgs
19.	SOV/Pr/GN/AN	70.1	1 named lg

However, it is not strictly required that this should happen across the different verb order types, because the number of languages of the three types differs considerably in the Appendix. Thus, there are 23 named VSO languages (18.9%), 41 named SVO languages (33.6%), and 58 named SOV languages (47.5%). Hence, a relative size prediction which holds within each of these three types might conceivably be offset across the types due to the disparity in the available number of languages. The fact that the predictions do nonetheless hold across SOV and SVO languages is, of course, interesting.

Let us now compare types 7 and 5 for cross-category harmony:

7. VSO/Po/GN/AN	1 named lg	5. VSO/Po/NG/NA	0 named lg
i/ VSO & Postp	$^{1}/_{23}$ = 4.3%	i/ VSO & Postp	$^{1}/_{23}$ = 4.3%
ii/ Postp & N-final	$^{28}/_{36}$ = 77.8%	ii/ Postp & N-initial	$^{6}/_{39}$ = 15.4%
iii/ VSO & N-final	$^{3}/_{22}$ = 13.6%	iii/ VSO & N-initial	$^{15}/_{23}$ = 65.2%
Total =	95.7	Total =	84.9

Of these two potentially attestable language types, the one that is actually attested has the higher cross-category harmony figure. Notice, however,

that according to the above table for minority SVO and SOV languages, the type 5 figure of 84.9 should justify at least one attestation. I attribute its absence to the fact that the total number of VSO languages in the sample is so small. We are dealing here with minority language types. Given a far larger sample of VSO languages I predict that further such languages will occur, and that their distribution will reflect our cross-category harmony calculations.

We have given detailed consideration to fourteen of the sixteen attested types in Appendix II, we have established CCH correlations for all the named languages in the Appendix, and we have also considered the data in the 30-language sample. CCH seems to make predictions about relative quantities of languages which are correct, even for the small numbers of languages within the minority types. Notice finally, however, that types 12 and 14 do not obey it. Compare them with the SVO types which they should most resemble in terms of CCH, 10 and 16:

10.	SVO/Pr/NG/AN	8 named lgs
12.	SVO/Pr/GN/NA	1 named lg
14.	SVO/Po/NG/AN	1 named lg + other Daghestan lgs
16.	SVO/Po/GN/NA	9 named lgs + most Mandingo and Voltaic lgs

All these SVO languages are noun-medial. According to CCH 10 and 12 with prepositions should be more or less equal in distribution, as should 14 and 16 with postpositions. The gross disparity in distribution correlates with the fact that 12 and 16 provide the only exceptions to universals e) and f). If we therefore propose these languages as exceptions to e) and f), replacing universals C and D by e) and f) in the process, we have an automatic explanation for the distributional facts. Alternatively, we can retain the exceptionless C and D, and set up types 12 and 14 as the unique exceptions to the principle of cross-category harmony.

Section (F): Theoretical Conclusions and Explanatory Hypotheses

I have proposed a total of six implicational universals distinguishing attested from non-attested languages, and one distributional generalisation accounting for the relative size of the attested types in Appendix II. I derive the following theoretical conclusions from the fact that such a large body of data submits so readily to generalisations which are essentially simple and regular.

Conclusion One: The Distributional Generalisation

Languages do NOT strive to serialise their modifier-modified (operator-

operand) elements in a consistent left to right, or right to left order, as in Vennemann's natural serialisation principle. Modifiers are both preposed and postposed in all but type 1 VSO/Pr/NG/NA and in all but the rigid SOV languages of type 23 SOV/Po/GN/AN. Instead, languages strive to achieve a balance between the number of preposed and the number of postposed modifiers which they have in each modifier category. The greater this "cross-category harmony", the greater is the number of attested languages. This distributional generalisation holds irrespective of which pre- and postposed modifiers are permitted to co-exist in a language by implicational laws. Hence, it is sensitive only to numbers of pre- and postposed modifiers, and not to their nature.

Conclusion Two: Greenberg's Statistical Universals

Greenberg's statistical word order universals are NOT a significant part of universal grammar. They fail to capture generalisations, and can all be replaced by our one distributional generalisation. Greenberg has tried to fit universal regularities of two distinct kinds into one and the same logical format. As a result, he not only fails to notice the distributional generalisation, he necessarily creates statements with many exceptions. The implicational universals of section (D) relate a small number of properties to one another. The presence of some property or properties P is claimed to guarantee the presence of Q, and hence the absence of -Q, in all type P languages. The absence of the -Q properties is thereby claimed to follow from whatever properties exemplify P, these properties being considered uniquely sufficient to explain the absence of -Q. Logically, our distributional generalisation is still implicational: if some group of languages A has more cross-category harmony than some group of languages B, then the quantity of A will exceed that of B. But the values of P and Q are here totally different. We are not now relating adjective order to genitive order, or verb order to adjective and genitive orders, i.e. we are not relating a limited number of specific word orders to one another in some exemplifying language. We are making a correlation between, on the one hand, the overall preposing/postposing balance between all the word orders of some language type, and, on the other hand, some relative quantity of exemplifying languages throughout the world. Any attempt to reduce such a correlation to a type of implicational statement which is designed to predict the presence and absence of a small number of co-occurring word orders is bound to fail, for two general reasons. Firstly, in any implicational statement between particular single properties it is impossible to capture the conditioning factor in the distributional generalisation, namely the preposing/postposing balance for the language as a whole. The best that can be done is to pick some particular properties whose positive P and Q values are very harmonic (e.g. SOV and postpositions), and whose P &-Q values (SOV and prepositions) are disharmonic and have a low frequency of occurrence. In

this way, statistical distributional statements can be made to approximate to implicational statements between single properties in which P &-Q is totally unattested. But clearly to make a separate statement for every harmonic and disharmonic pair in this way is highly uneconomic and fails to capture the overall regularity. Language quantities reflect the preposing/postposing balance for the language as a whole, regardless of the particular constructions which the language has, and regardless of whole language type (VSO or SOV etc.). To enumerate statistical correlations between particular properties is therefore to fail to appreciate this overall conditioning factor, and to fail to capture generalisations both across properties and across language types. Secondly, the only type of distributional statement that Greenberg can make with these statistical implications is one which distinguishes majority (or regular) from minority (or exceptional) types. But these minority types should not be exceptions to anybody's predictions. They are attested, and their minority status is perfectly regular according to CCH. And yet there is no way in which any other finer distributional predictions can be made within this format. In effect, Greenberg relies on the very existence of exceptions in order to make distributional distinctions. Since the existence of too many exceptions is unpalatable, only majority types can be distinguished from minority types, in which the "exceptional" P &-Q languages are few. Only by admitting of higher percentages of exceptions could finer distributional predictions be made. But even then these distributional statements would become progressively harder to state by correlating particular properties with one another, because of the increasing absence of any regular correlation with particular properties for the medium-sized language types. In short, Greenberg's statistical universals are a mistake. They should have been formulated with very different values for P and Q, which would have converted them into non-statistical distributional statements. In section (E) we argued that only one such statement is necessary, CCH.

Conclusion Three: The Total Absence of Possible Co-occurrence Types

Appendix II shows that one third of the conceivable word order co-occurrence possibilities for just four properties are totally unattested. The balance between attested and non-attested types is, moreover, far from random. They can be readily distinguished on the basis of six simple implicational laws, which consist in effect of three mirror-image pairs A/B, C/D, and E/F. These laws are exceptionless, or non-statistical, as are most of Greenberg's universals and the ease with which they can be formulated should encourage us to look always for exceptionless statements, and to resort to exceptions only if absolutely necessary. To elevate statistical statements into the norm, as in Vennemann's "co-occurrence tendencies", is to fail to explain the absent types, to miss the distributional generalisation, to create huge numbers of

exceptions, and to misrepresent the balance between statistical and non-statistical statements in Greenberg's work. And given that Greenberg's statistical universals are misformulations for distributional regularities which should have been stated in an altogether different, and non-statistical way, it is unfortunate that Vennemann chose to misrepresent Greenberg in favour of the mistaken type. The resulting tolerance of exceptions that has now become commonplace has allowed theories to develop, such as the natural serialisation principle, which have very little empirical support. Now obviously Greenberg's sample still contains only a sample of all the world's languages, and quite possibly there are exceptions to the universals proposed in this paper. In that case they will have to be reformulated. But at each stage in the research process we must be faithful to the available data.

Conclusion Four: The Need for Multi-valued Correlations

The six implicational universals proposed in this paper all have one thing in common. They relate at least three properties to one another, never just two. Thus, the implication AN → GN holds only in languages which are SOV, etc. It is interesting that of the five implications which Greenberg proposes in order to account for the data of Appendix II ((2), (3), (4), (5) and (17)) only one relates more than two properties to one another ((5)), and only that one has no exceptions.

Conclusion Five: The Reality of Three-way Typologies Defined by Unilateral Implications

We have seen ample evidence in section (D) for the existence of three-way word order co-occurrence typologies in language. The three co-occurrences of, for example, AN → GN in SOV languages are precisely those three of the four mathematical possibilities which match the P & Q, -P & Q and -P &-Q values of a unilateral implication. Notice, however, that in Vennemann's operator-operand schema, the logical status of all the unilateral implications has been changed. They are now bilateral, or reversible, implications permitting just two co-occurrences, P & Q and -P &-Q. In effect, P & Q languages serialise their operator-operand structures in one order, while -P &-Q languages serialise them in the reverse order. There is no place in this system for the third type, -P & Q, whose existence we have seen to be both valid and productive. Hence, there is a systematic failure in Vennemann's work to distinguish -P & Q from P &-Q co-occurrences, and this accounts for most of the exceptions to his principle. What is strange is that Vennemann seems unaware that he is changing the logical status of Greenberg's implications. Following Greenberg, he still assumes that the position of the verb relative to the object is the antecedent property in a set of non-reversible implications, and he goes out of his way to explain why the verb-object

order should be implicationally antecedent in this way. It is because children acquire verb-object sequences before any of the other operator-operand structures that the trend is set for the relative ordering of the latter. And in Vennemann's historical theory, the verb-object order is the first to change, with the newly acquired verb position triggering the subsequent acquisition of all the implicationally dependent operator-operand structures. But by, unwittingly, changing Greenberg's unilateral to bilateral implications Vennemann introduces a serious problem for his historical theory. In a bilateral, reversible implication, P and Q imply the co-occurrence of one another (i.e. P $\rightarrow$ Q and Q $\rightarrow$ P). Historically, therefore, the acquisition of either P or Q could trigger the subsequent acquisition of the other. It is therefore no longer necessary that the unique implicational antecedent in Greenberg's unilateral formulation, P, be the first property to be acquired historically, since P and Q, according to Vennemann's formulation, are equally capable of implying the co-occurrence of the other. Any one operator-operand structure, if acquired first, could therefore trigger the subsequent acquisition of all other operator-operand structures serialised in the same way, (but cf conclusion seven).

Conclusion Six: The Number and Nature of "Basic" Language Types

Despite the reality of Greenberg's unilateral implications, however, I believe that Vennemann is right about the number of basic language types which exist. There are just two. But he is wrong in the way that he describes the difference. The truth probably lies somewhere mid-way between Greenberg and Vennemann. What distinguishes the two language types is that each obeys its own characteristic set of unilateral implications. From the discussion in section (D) we see that unilateral implications exist both between the modifiers of one and the same category (adjective and genitive order in relation to the noun), and across the modifier categories (e.g. from VSO to certain specified noun-modifier orders, from Prep to noun-modifier orders, and so on). There are just two implications defining co-occurring adjective and genitive orders, NA $\rightarrow$ NG and AN $\rightarrow$ GN. It transpires that the former correlates with prepositions, and the latter with postpositions (with just two named exceptions):

e) Prep $\rightarrow$ (NA $\rightarrow$ NG)
f) Postp $\rightarrow$ (AN $\rightarrow$ GN)

Let us therefore refer to our two language types as "prepositional" and "postpositional" languages respectively. In Hawkins (1976) and Hawkins (in preparation) I demonstrate that the mirror-image implications NA $\rightarrow$ NG and AN $\rightarrow$ GN are just two examples of a whole set of opposing implications which characterise prepositional and postpositional languages respectively. The effect of these different implications is to

create opposing word order co-occurrences which are uniquely characteristic of each language type, such as AN & NG for prepositional languages, and NA & GN for postpositional languages. Such opposing implications exist not only between modifiers of the noun, but between modifiers of the verb as well. For example, it happens frequently in postpositional languages that the object precedes the verb whereas whereas the adverbial follows (OV & VAdv), whereas prepositional languages regularly have the object after the verb, while allowing the adverbial before the verb (VO & AdvV), generally in sentence-initial position. The verb-peripheral orders, VSO and SOV, also function as implicational antecedents to the same noun-modifier implications as Prep and Postp respectively:

A VSO → (NA → NG)
B SOV → (AN → GN)

This parallelism is a consequence of the fact that only two basic language types exist, each with its own set of noun-modifier implications. However, although there are just two sets of noun-modifier implications, and just two adposition orders (languages being either prepositional or postpositional, but rarely both at once (i.e. adposition-medial)) there are more than two basic verb positions. And although verb-peripheral order correlates with noun-modifier implications, verb-medial order (SVO) does not. Hence, if we elevate verb position to type status, we face the anomaly that not all verb positions give principled correlations. But each adposition order does correlate, and hence this is a better type designator. Notice, however, that the two noun-modifier implications, NA → NG and AN → GN, overlap with regard to the noun-initial and noun-final co-occurrences NA & NG and AN & GN. It is these overlapping noun-peripheral orders which we have seen to co-occur with the disharmonic SOV & Prep and VSO & Postp. The number of overlapping co-occurrences between the total sets of prepositional and postpositional type noun-modifier co-occurrences presented in Hawkins (1976) and (in preparation) is again limited to just two co-occurrences, the noun-final and noun-initial orders. Otherwise the noun-modifier co-occurrences of prepositional languages are all incompatible with those of postpositional languages.

In addition to having these characteristic "category-internal" modifier co-occurrences, prepositional and postpositional languages also obey the "category-external" or cross-category constraints defined by implications C, D, E and F:

C Prep & (VSO v SOV) → (NA ↔ NG)
D Postp & (VSO v SOV) → (AN ↔ GN)
E Prep & (SVO v VSO) → N-final
F Postp & (SVO v VSO) → N-initial

In all four cases verb-peripheral and noun-peripheral orders, in

conjunction with the specified adposition order, exert strong co-occurrence requirements on noun-modifier and verb-modifier orders respectively. It is precisely these "operand-peripheral" orders, such as noun-initial and noun-final, which are the overlapping orders in the two sets of category-internal implications. Their co-occurrence with prepositions or postpositions therefore guarantees unique co-occurrences for each language type. But notice that there is no strict requirement on postpositional languages that they must have non-VSO order, and none on prepositional languages that they must have non-SOV order. The minority status of VSO & Postp and SOV & Prep is entirely a function of our distributional generalisation, and does not reflect any logical violation.

Summarising, there exist two "basic" word order types throughout the world, which may be most conveniently referred to as "prepositional" and "postpositional" languages. The difference between them is more abstract and complex than in Vennemann's theory. Each type obeys firstly a different set of unilateral implications holding between modifiers of one and the same category (NA $\rightarrow$ NG etc.). Secondly, each obeys a set of cross-category implications holding between verb-modifiers and noun-modifiers (at least one of which is "operand-peripheral") in conjunction with the appropriate adposition specification. The divisibility of our six implications into three mirror-image pairs (A/B, C/D and E/F) is a further consequence of this basic two-way typology.

Conclusion Seven: Historical Theories of Word Order Change

The logic of Vennemann's historical theory of word order change is typical of many (cf also the work of Lehmann in the references). A language first acquires some property P (VO or OV order), which then triggers the later acquisition of whatever properties Q are dependent on P by a universal implication P $\rightarrow$ Q. By explaining why P arises in the first place (in Vennemann's theory phonological reduction causes subject-object ambiguity in SOV languages which in turn causes the verb to shift between subject and object (SVO) as a disambiguator) one has at a stroke explained sweeping word order changes throughout the language as a whole, in the form of all Q properties which are implicationally dependent on P. P exists first without Q ($*$P &-Q), so Q must be acquired in order to reintroduce universal consistency. In Vennemann's theory P establishes a new operator-operand serialisation pattern with which all other word orders are then supposed to become consistent. The findings of this paper, however, argue against the whole logic of this form of historical explanation, as well as against Vennemann's particular theory. Firstly, such theories rely crucially on the existence of a language stage with P &-Q, i.e. with an exceptional and inconsistent co-occurrence which motivates change. But the improved implications of section (D) have no exceptions, i.e. there exist no P &-Q co-occurrences in the

synchronic data. As a result, the assumption that P &-Q co-occurrences can be trigger mechanisms of change is quite implausible, for we cannot, when invoking within a historical context language universals which are derived from synchronic evidence, depart from this evidence in explaining change. If the synchronic evidence suggests mere co-occurrence tendencies, then the assumption of inconsistency as a mechanism of change is possible. But to the extent that the synchronic evidence is compatible with non-statistical co-occurrence constraints, the whole historical logic falls. I also demonstrate in Hawkins (1976) and (in preparation) that word order changes in the Germanic family do not violate universal word order implications (including those we have justified in this paper), but change within the synchronically attested constraints. Notice secondly that a crucial ingredient in Vennemann's theory seems to be the development of SVO word order within a language, as the trigger property from an SOV stage. But SVO is precisely the verb position which is typologically ambivalent and has no implicationally dependent Q properties, and hence no trigger potential.

Thirdly, there are numerous internal contradictions within Vennemann's theory. To the extent that his universals are sufficiently weak that languages may regularly violate them and develop P &-Q co-occurrences, why should these same universals then become so strongly operative as to effect a total restructuring of the syntax of the language by introducing all the properties which are implicationally dependent on P? Vennemann wants to have it both ways. He wants to get an inconsistent P &-Q into a language by first violating his universals, and then he wants to have these same universals introduce regular and consistent co-occurrences. But given the widespread syntactic changes that are supposed to ensue from regularising P &-Q co-occurrences, the necessary strength of the P $\rightarrow$ Q implication sufficient to reintroduce consistency is quite at variance with its weakness in allowing inconsistency to arise in the first place. Fourthly, I have already pointed out that any of Vennemann's operator-operand orders, and not just the verb-object position, could be a historical trigger according to his bilateral formulation of the implications. Notice a fifth problem connected with his reformulation. Logically, there are two ready ways in which the inconsistency of P &-Q could be resolved. The language could move on to P & Q, but it could equally well return to the consistent -P &-Q. Notice further that P (VO) is, according to Vennemann's theory, a single property in a whole language full of -Q word orders. If P &-Q is to be resolved in favour of P & Q, then the whole language must change. If it is to be resolved in favour of -P &-Q, only the verb need shift position, back to OV. Worse yet, when P arises historically, it does not just spring into the language in toto, but co-exists for a long period alongside -P. Now according to Vennemann's bilateral formulation, his one language type (operator operand) obeys the implications P $\rightarrow$ Q and Q $\rightarrow$ P, while the other type (operand operator) obeys the reverse -P $\rightarrow$ -Q and

-Q → -P. Hence, any pull which P (VO) exerts in favour of the acquisition of Q structures at the P &-Q stage, will simply be offset by the implication -P → -Q radiating from the co-existing -P (OV) in favour of the retention of -Q. And any increase in the P structure relative to -P should be offset by the implication -Q → -P forcing the retention of -P. To claim therefore that the existence of P &-Q structures explains the historical rise of positive Q word orders is to invoke a single implication as a mechanism of change, P → Q, when there are two other implications within the same theory, -P → -Q and -Q → -P, which contradict this development, and which force a language to retain the properties which it has. But most important of all, the whole trigger-chain logic of Vennemann's theory depends crucially on the existence of exceptional P &-Q language stages. By substituting exceptionless implications for loose synchronic co-occurrence tendencies we therefore remove the cornerstone of this historical logic, which we have shown to be self-defeating anyway.

Some Explanatory Hypotheses

My chief concern in this paper has been to establish word order generalisations of greater descriptive adequacy than those currently available. It is still quite premature to propose any authoritative explanation for these principles until a lot more universal data has been collected, and descriptive patterns formulated. Nonetheless, I shall conclude with some brief thoughts on the kinds of explanatory principles which I think underlie our six implicational universals and the distributional generalisation.

I see the distributional generalisation as a consequence of two factors. It points firstly to the validity of a semantic-syntactic parallelism between the verb and its modifiers, the noun and its modifiers, and the adposition and its modifiers. Within the three categories, S, NP and adposition phrase (AdpP), therefore, the constituents V, N and Adp can be regarded as semantic head or operand constituents, with the other accompanying constituents functioning semantically as modifiers or operators, essentially as in Vennemann's semantic analysis, except that our universal evidence suggests that the subject should be regarded as a verb-modifier just like all the other arguments of the verb. I see evidence for the reality of this schema in the very fact that our distributional generalisation refers to an organisational balance across all three categories. Secondly, this kind of ordering balance of the head constituents in relation to their modifiers seems to be naturally explainable in terms of analogy. Thus, the preposing and postposing pattern within one category generalises to another both as a result of the syntactic-semantic similarity between the two categories, and of a natural tendency observable throughout language for like elements to be treated in a like manner. To the extent that the preposing/postposing ratio of modifiers of one category moves away from that of the others,

the language comes under increasing pressure to reintroduce a balance. Since there is no absolute prohibition against specific co-occurrence types in this context, it becomes possible, though by no means necessary to invoke a trigger-chain effect here across the different categories. Thus, a movement of the verb relative to its modifiers may indeed trigger a movement of the noun within the NP - but relative to a set of co-occurrence possibilities which are universally permitted for that (prepositional or postpositional) language type. Alternatively, both may shift together. The more disharmonic a language is or becomes, therefore, the greater is the internal pressure to reintroduce an analogical preposing/postposing balance across the categories.

There is also a historical dimension in this principle. The decreasing quantities of progressively disharmonic languages across the globe on synchronic evidence can be seen as a natural consequence of the fact that every one language in the course of its evolution is harmonic for greater periods of time than it is disharmonic, with length of time in fact correlating with degree of harmony. If this is so, a synchronic sample of languages at any one point in time would naturally reflect in quantities of languages the amounts of time during which any one language was respectively harmonic and disharmonic. This historical hypothesis can not be checked directly since even the best recorded of languages provides only a relatively limited time-depth for inspection. However, some historical evidence, apart from the synchronic evidence, for correlating language quantities with time can be seen when comparing whole language families in evolution. In Hawkins (1976) and (in preparation) I show, using historical data from Germanic, how the daughter languages which introduce disharmony first, or to a greater extent, in the early periods, are the first to resolve it. Interestingly, modern English, which has SVO & Prep and entirely preposed noun-modifiers except for NRel and both GN/NG, is extremely disharmonic at this point in time and can be predicted (on comparative synchronic evidence) to become more harmonic in the not too distant future. But exactly how this will be done, whether by shifting the verb to the right in the sentence, or the noun to the left within the NP, can not be predicted.

The explanation for the implicational regularities of section (D) is, I think, clear in outline, if not in detail. There are going to be two basic ingredients in the explanatory package. Consider again universal f) Postp $\rightarrow$ (AN $\rightarrow$ GN), and recall the three noun-modifier co-occurrences permitted by the implication AN $\rightarrow$ GN:

i/	P & Q	(Postp) & AN & GN
ii/	-P & Q	NA GN
iii/	-P &-Q	NA NG

By far the majority of postpositional languages are of types i/ or ii/ with

at least one noun-modifier preposed and harmonic with Postp. Of these three co-occurrences we see, however, that the genitive is harmonic with Postp in two cases (GN: i/ and ii/), and the adjective just in one (AN: i/). In other words, the genitive exerts a stronger requirement than the adjective towards being harmonic with the ultimately conditioning category-external property Postp. The adjective is therefore more variable and instable than the genitive. It can stand in a disharmonic order relative to Postp while the genitive is still harmonic. And in the single case that the genitive is disharmonic (NG: iii/) the adjective must also be disharmonic. The two factors which account for this implicationally defined typology are therefore going to involve firstly inherent semantic-syntactic differences between adjective and genitive (explaining the relative instability of the former), and secondly the fact of historical change. Thus, given that a postpositional language is moving, for whatever reason, towards noun-modifier postposing, the first modifier to shift is going to be the syntactically and semantically more variable one, the adjective. The facts in prepositional languages are exactly parallel:

e) Prep $\rightarrow$ (NA $\rightarrow$ NG)

i/	P & Q	(Prep) & NA & NG
ii/	-P & Q	AN NG
iii/	-P & -Q	AN GN

Again, the adjective may be disharmonic while the genitive is still harmonic (ii/), and the disharmony of the genitive guarantees the disharmony of the adjective (iii/). The historical changes do not have to be unidirectional according to this account. Languages may move in either direction, from i/ through iii/ and from iii/ through i/. Thus, the noun-modifiers in all the Germanic languages have gone from postposed to preposed to postposed and back to preposed again (cf Hawkins (1976) and (in preparation)). But at each stage they obey the co-occurrences defined by their prepositional implication. Whatever the direction of movement, the genitive is going to exert a stronger pull towards cross-category harmony than the adjective.

Now, what could explain the instability of the adjective? Presumably the fact that the adjective is syntactically and semantically less complex than the genitive. The latter consists of a whole NP modifying another NP in conjunction often with some genitive marker. Thus, the genitive may consist of a nominal referring predicate plus all the syntactic-semantic material which may occur within an NP, adjectives, determiners etc. But the adjective is a single descriptive predicate, and is both less complex and morphologically and syntactically shorter than a genitive. Any language, therefore, which is going to change the order of its noun-modifiers can reorder its adjective with greater ease and with less drastic restructuring of the NP, than by moving the whole genitive expression. Given the gradualness of language change, the shifting of

adjectives away from the harmonic order before genitives is therefore a natural consequence of the relative semantic-syntactic complexity distinguishing these two modifiers. Genitives are going to hang on longest to the harmonic order.

The explanation for the implications A and B in which the same noun-modifier co-occurrences are defined on peripheral verb position (VSO or SOV) rather than peripheral adposition order is essentially similar to that we have just proposed. The remaining implications E (Prep & (SVO v VSO) → -N-final) and F (Postp & (SVO v VSO) → -N-initial) are perhaps to be explained in terms of the extreme disharmony between adposition order and noun-modifier orders, although we must then explain why these principles are relative to SVO and VSO languages only.

Finally, I attribute the fact that there are ultimately just two "basic" language types, obeying different implicational laws, to the fact that there are only two basic positions that a modifier can adopt in relation to its head: before or after. Where the number and nature of modifiers is semantically and syntactically limited (as with adposition modifiers) we get just the two possible orders: Prep and Postp. Where the number of modifiers is much greater (with the noun and the verb) we get a whole set of three-way co-occurrences between each pair of modifiers, reflecting the relative semantic-syntactic stability of the members of each pair.

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